# MODERN ASTRONOMY

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BEING SOME ACCOUNT OF THE REVOLUTION OF THE LAST QUARTER OF A CENTURY


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ASTRONOMICAL ARMY

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BY

A COMRADE

IN THE REGULARS

# PREFACE

In the following pages an attempt is made to show how powerfully Astronomy has been affected by the scientific events of the last quarter of a century, and especially by the invention of the photographic dry-plate. So great are the changes in method which either have actually been made, or are rendered immediately possible, that the word Revolution is used in referring to them; and though so strong a word may cause surprise even to astronomers, who might be expected to be fully conscious of the magnitude of such changes, it must be remembered that their attention has been much occupied by the additional work involved; they may easily have been swept along a considerable distance in twenty-five years without realizing how far they have come.

My object is accordingly rather to point out the nature and magnitude of the changes than to give a complete account of them. I

would represent myself as conducting a party of visitors over an establishment where large additions and improvements have recently been made; not stopping to examine everything, and perhaps dwelling unduly over things with which I am personally most familiar.

The book owes its origin to three lectures given at the Royal Institution in February, 1900; but what was then said has been considerably expanded and added to.

I am indebted for permission to reproduce illustrations to the Royal Astronomical Society, the Astronomer Royal, Sir Robert Ball, Prof. E. E. Barnard, Sir David Gill, Mr. McClean, Prof. E. C. Pickering, Prof. R. A. Sampson, and Dr. Max Wolf.

H. H. TURNER.

UNIVERSITY OBSERVATORY,
OXFORD, *December* 20, 1900.

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# Section I

## MODERN INSTRUMENTS

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## MODERN INSTRUMENTS

**An Astronomical Revolution**

During the last quarter of a century there has been a revolution in almost all departments of Astronomy, theoretical and practical. Before 1875 (the date must not be regarded too precisely), there was a vague feeling that the methods of astronomical work had reached something like finality: since that time there is scarcely one of them that has not been considerably altered, or is not on the point of alteration; and entirely new departures have been taken. Such a sudden development in a science which had apparently reached a "sticking-place" justifies a brief review of the situation.

**Previous to 1875**

Two or three quotations will first be given to show that the feeling above referred to (that we had almost come to the end of our

astronomical resources) really existed. We have had such good cause to think differently of late years that this state of mind has been forgotten—probably many would deny that it ever existed. But it was so real that it found its way into print, and so can be recalled. Take first the words of a President of the Royal Astronomical Society in 1887.

**Lunar Theory**

"The belief has been prevalent," he says, "that the mathematical portion of the treatment of lunar theory has been worked out, and that there was no scope for the display of mathematical skill, or the employment of modern mathematical methods."[1] These are the deliberate words of Dr. Glaisher in presenting the gold medal of the Society to Dr. G. W. Hill, who had swept away this misconception by his brilliant investigations, so that the President went on to characterize his work as the "dawn of a new day in the history of the lunar problem."

**Double Stars**

The same belief, that the field had been worked out, was prevalent in practical astronomy; and an excellent illustration of the fact is given by that fine observer of

[1] *Mon. Not. R.A.S.* vol. xlvii. p. 217.

double stars, Mr. S. W. Burnham. In his recently published catalogue [1] he writes:—

"For many years prior to 1870 it seems to have been practically accepted that the field for the discovery of new pairs had been substantially worked out by the Herschels and the Struves, and that so little had been overlooked by these eminent pioneers in this work that there was little chance for later observers to make many important additions. . . . The late Rev. T. W. Webb, author of *Celestial Objects for Common Telescopes*, one of the most eminent English amateur astronomers, in a letter written to me in 1873, after the publication of my first three catalogues, said: 'It will hardly be possible for you to go on for any great length of time as you have begun, because the number of such objects is not interminable, and every fresh discovery is one less to be made; still, what you have already done is so much more than any man now living has accomplished that your high position as an observer is fully secured.' Since that time more than 1,000 new double

[1] Vol. I. of the Yerkes Observatory publications, p. xiii. of the "Introduction."

stars have been added to my own catalogues, *and the prospect of future discoveries is as promising and encouraging as when the first star was found with the six-inch telescope.*"

The italics for the last sentence are mine; it seems well to emphasize this opinion of so capable an observer and discoverer. It may further be remarked that Mr. Burnham has demonstrated the vast possibilities for future discoveries in spite of a considerable limitation which he has himself imposed on the field of work: nine-tenths of what *were* called double stars he rejects as irrelevant, because the components are too far apart to be physically connected. If he had adopted the standard of the Herschels and Struves he could have "increased the number of pairs to hundreds of thousands by sweeping with a very moderate aperture." This limitation greatly enhances the value of his discoveries and his testimony: the field of work was by no means thoroughly explored—a beginning had in reality scarcely been made; but the astronomers of the middle of the century were content to assume that their predecessors had done nearly all that could be done.

**Birth of Astrophysics**

Take again the words of Sir William Huggins in a recent article[1] in which he gives an account of the birth of "The New Astronomy." He tells us first how, about the middle of the century, he decided to devote himself to observational astronomy,—"after a little hesitation, for I was strongly under the spell of the rapid discoveries, then taking place, in microscopical research in connection with physiology." He accordingly built himself an observatory, obtained what was at the time a very fine telescope, and did some excellent work. But then he tells us plainly (the italics are mine):—

"I soon became a little dissatisfied with the *routine character of ordinary astronomical work*, and in a vague way sought about in my mind for the possibility of research upon the heavens in a new direction or by new methods. It was just at this time, when a vague longing after newer methods of observation for attacking many of the problems of the heavenly bodies filled my mind, that the news reached me of Kirchhoff's great discovery of the true nature and the chemical constitution of the

[1] *Nineteenth Century*, June, 1897.

sun from his interpretation of the Frauenhofer lines. The news was to me like the coming upon a spring of water in a dry and thirsty land. Here at last presented itself the very order of work for which, in an indefinite way, I was looking—namely, to extend his novel methods of research upon the Sun to the other heavenly bodies."

How Sir William Huggins took advantage of the opportunity is well known. We must date the birth of a new science, which is now called Astrophysics, from this time—about 1860. This is a good deal earlier than our date 1875; but we need not, therefore, abandon the latter as a reference epoch; for it was not until about that time that the invention of the dry-plate gave the spectroscope entirely new powers.[1] Indeed, without disparaging the importance of what was done with the spectroscope before that time, we may remark that some eminent astronomers did not accept it as a serious and

[1] Sir William Huggins in the article quoted, writes: "The great and notable advances in astronomical method and discoveries by means of photography since 1875, are due almost entirely to the great advantages which the gelatine dry-plate possesses for use in the observatory, over the process of Daguerre, and even over that of wet collodion."

permanent addition to astronomical equipment. "When the novel and entertaining observations with the spectroscope have received their natural abatement," said one of them, a little jealous for the welfare of older work, "and have been assigned their proper place, it is to be hoped that some of the powerful telescopes recently constructed may be devoted to the observations of satellites.[1] This was in 1878, and the words are not quoted as an instance of individual error of judgment, but as a reflection of the spirit of the times in the frank utterance of an eminent man. The important lines of work in astronomy had come to be considered as settled, and any interference with them was regarded with a little impatience.

**Routine**

The routine work which wearied Sir William Huggins had become very strong; it had, perhaps, gained in strength, rather than lost, in the years between 1860 and 1875. The transit-circle had come to be regarded as the instrument of the professional astronomer, who could not do better than observe with it the positions and motions of the planets and fixed

[1] *Mon. Not. R.A.S.* vol. xxxix. p. 308.

stars, on established lines. He would supplement such work with the aid of the equatorial for double stars and faint objects; but the equatorial was regarded rather as the instrument for amateurs, who should make drawings of planets, comets and nebulæ. The size of a refracting telescope was supposed to have nearly reached its limit at about 18 or 20 inches. Something had already been done in photography, and some observations of great value made with the spectroscope; but there was a feeling that these new departures were not likely to lead very far. We get a notion of the state of affairs by a glance at the list of awards of the gold medal of the Royal Astronomical Society for more than a quarter of a century. From 1848 to 1879, from the recognition of the discovery of Neptune to that of the discovery of the satellites of Mars, the awards are nearly all for achievements on well recognised lines rather than for original discovery; work of conspicuous merit, but lacking a little in that novelty to which we have recently become well accustomed. Ill-natured critics, if such there be, might almost have accused astronomers of a gentle drowsiness.

But we have recently been supplied with numerous excellent reasons for the most wakeful activity. Not only Hill but Gyldén and Poincaré have proposed new methods of attacking the great problems of lunar and planetary theory. G. H. Darwin began in 1877 his patient work of attempting to unravel the history of our solar system, and thus organized a new department of theoretical astronomy which had been represented previously only by Laplace's sketchy speculation called the Nebular Hypothesis. In practical astronomy new instruments of precision have been suggested to take their place alongside the transit circle; the size of telescopes has increased by leaps and bounds, and the important step has been taken of placing large telescopes in climates which enormously multiply their efficiency; the spectroscope has created a new astronomy of its own, and finally the invention of the dry-plate has provided astronomy with a weapon whose use seems to be inexhaustible. In all these directions there have been vast additions to astronomical work and responsibilities, and were the shade of an astronomer who died in the middle of the century to re- **Since 18'**

visit his familiar haunts, he would scarcely recognise many of the pursuits of his successors.

**Illustration from the Royal Observatory, Greenwich**

A particular example of this general course of events is afforded by the history of our own national Observatory at Greenwich. It has twice undergone reconstruction in the present century: the first time in the years 1835–59 by Sir George Airy, who fitted it admirably for the "routine work" characteristic of the middle of the century; the second time by Mr. Christie, the present Astronomer Royal, who has equipped it for playing its part in "modern astronomy."

**The reconstruction by Airy, 1835–59**

When Airy succeeded Pond as Astronomer Royal in 1835, he set himself to provide the Observatory with first-class instruments, and to organize its work on a thoroughly sound basis of routine, so that it should be in the front rank of observatories. He set up a first-rate transit-circle for meridian work, which has never been surpassed, if equalled; an altazimuth for observations of the Moon at times when it could not be caught on the meridian; a reflex-zenith tube, and finally the south-east equatorial of 13 inches aper-

ture. Reorganization was extended also to the staff and buildings; and after more than twenty years' work he announced that the reconstruction was complete—so complete that not a single person was still employed, or instrument used, which had been there when he came.[1]

He had, indeed, brought the equipment well up to date, and had every reason to be well satisfied with the result. It would have been interesting to see what would have happened,

[1] Extract from the Astronomer Royal's Report for 1859:—

"There is not now a single person employed or instrument used in the Observatory which was there in Mr. Pond's time, nor a single room in the Observatory which is used as it was used then. In every step of change, however, except this last, the ancient and traditional responsibilities of the Observatory have been most carefully considered; and in the last (viz., the erection of the S.E. equatorial), the substitution of a new instrument was so absolutely necessary, and the importance of tolerating no instrument except of a high class was so obvious, that no other course was open to us. I can only trust that while the use of the equatorial within legitimate limits may enlarge the utility and the reputation of the Observatory, it may never be permitted to interfere with that which has always been the staple and standard work here."

if, for instance, the dry-plate had been invented just about this time or a little earlier, so that its influence on Astronomy was beginning to be felt. It would have been a sore trial for Airy to begin afresh just when he had completed his reconstruction; but we can scarcely doubt that he would cheerfully have done so. As it turned out, the next twenty years were so generally quiescent that his new instruments and staff were able to settle down into steady, useful work, and it would be difficult to overestimate the value of the work done with the transit-circle during this period, and of the natural corollary to it in the similar work carried out by Mr. Stone at the Cape of Good Hope. Of Airy's other instruments it must be admitted that the altazimuth and reflex-zenith tube did not turn out altogether satisfactory. As regards the equatorial (for which he almost apologised, hoping that its use would never be allowed to interfere with the staple work of observation with the transit-circle and altazimuth), it need not cause surprise that but little was done with it. It was not till 1873 that a spectroscope was obtained to be attached to it; the same year in which photography was recognised in the

Observatory by the mounting within its walls of the Kew photoheliograph for taking daily photographs of the Sun. In these new departures we may trace the influence of the present Astronomer Royal, then Chief Assistant.

**Recent Reconstruction**

When Christie succeeded Airy in 1881, he began a reorganization of the Observatory, much as Airy had done half a century before him. The transformation has taken him also some twenty years, and it is now very nearly complete.

I have classified the recent changes under four heads, typical of similar changes in the astronomical world generally. The classification is not wholly satisfactory, but it may serve, as that useful phrase goes, to fix the ideas.

In the first place, the position of the work for which Airy showed such concern, so far from being weakened, has been strengthened. Airy's altazimuth, for observing places of the Moon away from the meridian, to which he attached very great importance, was never very satisfactory, and the present Astro-

nomer Royal has substituted an instrument of a higher grade of precision, which is, in fact, a new transit-circle with additional features, and as a preliminary to describing it, we may recall the characteristics of the transit-circle in its ordinary form,—an instrument which has been already mentioned several times, and which is of the very first importance in the work of a great national observatory. The casual visitor is usually surprised to learn of its importance; he is prepared to be shewn the largest telescope in the Observatory as the chief item of interest, and receives with obvious bewilderment the explanation that work with large telescopes does not take a prominent place in the programme. But for the work of a national observatory this is certainly the case; the accurate shape of the pivots of the transit-circle is of far more importance at Greenwich than the size of the largest equatorial, and the reason will now be briefly explained.

**The Transit-Circle**

The transit-circle is a telescope mounted something like a gun with a firmly fixed carriage. It can be elevated properly by turning it round a horizontal axis, but the amount of

elevation required in the case of a gun is never very large, and this is a limitation which we must suppose removed in the case of the transit-circle, which can be pointed to stars at all elevations, and even downwards, so as to see star images formed in a mercury-trough—an important class of observation. But the fixed gun-carriage is a permanent limitation, at once the strength and the weakness of the instrument. It is its strength because in this way great steadiness is secured. The gun-carriage is represented by massive stone pillars on very firm foundations, and though these are by no means immoveable (it was found by Airy to his great astonishment that in spite of all his care to get a fixed support in this way, the massive piers themselves swung slowly backwards and forwards throughout the year, from some obscure cause, probably meteorological), yet their changes in position are small and can be allowed for. The instrument is virtually quite steady, and is used for determining the most accurate positions of the heavenly bodies.

The limitation is, however, a weakness, in that these positions can only be observed at

special times. The gun is, so to speak, permanently laid north and south, and only commands an enemy who places himself directly north or south: it could not be used on an army which refused to move on to this line at whatever range, but would be effective on one which was incautious enough to march past. Now, the heavenly bodies are continually marching past, owing to the daily rotation of the earth; and so each of them in turn comes within view of the transit-circle at some elevation, and at this opportunity the accurate observation of its position is made. To notice the moment when the opportunity occurs is indeed an observation in itself, and if we note at the same time the elevation given to the telescope in order to see the object, we have all the material necessary for specifying the place of the heavenly body on the celestial sphere.

**Its Uses** Almost all our knowledge of the movements of the planets and their satellites, as well as of the stars, is obtained in this way, by observations with the transit-circle. The orbits of the planets and their distances from us are traced from these observations, and when Neptune was discovered by the calculations of

Adams and Leverrier, from its disturbing effect on the planet Uranus, it was the transit-circle observations of the latter planet which formed the starting-point for their work. We learn whether the length of the day or year is changing: whether the earth's axis remains parallel to itself or is slowly changing its direction: and so we get glimpses into our history in ages past, and some idea of what we may expect in ages to come. By patient observation we test the accuracy and universality of the wonderful law of gravitation, and may hope in time to advance towards the explanation of its origin. These are a few of the ways in which the transit-circle helps us; and in all this work it is the accuracy of the measurement rather than penetrating power of the telescope, which is important: and so the steadiness of the instrument and the exact circularity of its pivots are more important than the size of its lens; and the stranger is puzzled when he is shown the chief instrument in the Observatory to find it a comparatively small one.

**The Altazimuth**

We have said that, for steadiness, the instrument must be mounted on firm and

massive piers—the gun-carriage is to be fixed as rigidly as possible. Attempts to remove this limitation have generally failed; and a conspicuous instance of this is to be found in the fact that Airy, who designed a perfectly satisfactory transit-circle, was unsuccessful in his altazimuth. The need for this latter instrument arose in this way:—there is a special importance attaching to observations of the Moon's place, as will presently be explained. Now the transit-circle, with its limited opportunities, did not get enough observations: it was for instance an aggravating, but in our erratic climate too frequent experience for the Moon to shine brightly until the time came for making the observation with the transit-circle, and then for clouds to come up and obscure her, only to roll away again when the opportunity was gone for that day. More than this, in the first and last quarters the Moon is so near the Sun that the time for the meridian (or transit-circle) observation is too near noon, and the Moon cannot be seen owing to the brightness of the surrounding sky. When the Sun has set and the Moon is shining against a dark sky, she is away in the west quite out of range of the

transit-circle. Hence Airy made the attempt to set up an altazimuth, which may be compared to a gun capable of being elevated at any angle as before, with a carriage which could be swung round to any point of the compass, the exact compass-point or "azimuth" being read off on a carefully divided horizontal circle. With this instrument the Moon could be observed at any time, if she would only show for a brief interval; but, oh! the cost to the observer on a fitful night! With the transit-circle there is disappointment enough for him when he comes to the instrument at the proper time (which may be at any hour of the night, let us say 3 a.m. on a cold morning) and the Moon hides her face behind the clouds at the critical moment; but he can at least go back to bed knowing that no other chance of observation will occur for 24 hours. But with the altazimuth there is a chance whenever the Moon will show herself for a sufficient interval, and he must wait and watch her "dodging in and out of the clouds on the horizon like a water-rat" as a patient observer once expressed it to me, hoping to secure his set of four observations at the times when she comes up for a rather longer breath

**Its Discomforts**

than usual; and perhaps after getting three of them finding them rendered useless by the impossibility of getting the fourth! Small wonder that the weary watcher was sometimes discovered next morning asleep on his uncomfortable and rather perilous perch!

Such discomforts might have been borne more cheerfully if the observations had been of sufficient value; but it must be accepted as a fact that while Airy's transit-circle was first-rate, his altazimuth was a failure. It gave considerable, but not sufficient, accuracy; and the observations have not helped the construction of lunar tables as it was intended they should. The accuracy of the transit-circle was lost when the carriage was put on a swivel so that the telescope had, in mechanical language, two degrees of freedom instead of one.

**The new Instrument**

But though this particular attempt to observe the place of the Moon off the meridian must be acknowledged a failure, the need of such observations is still pressing, and endeavours to obtain them must not be relaxed. There are several promising ways in which attempts may be made (I may perhaps express

the personal opinion that some photographic method will ultimately be found most successful), and of these the present Astronomer Royal has very appropriately chosen for trial a compromise between the transit-circle and altazimuth. There is no doubt of the excellence of Airy's transit-circle: cannot more of this accuracy be carried into observations in other azimuths? Christie's suggestion is very simple: instead of having permanent freedom for the carriage to swing in any azimuth, he proposed to lay the instrument permanently east and west, or north-east and south-west, or in some other selected direction; fixing the carriage just as firmly as in the case of the transit-circle. The instrument is, in fact, what is called in text-books a "transit-circle out of the meridian," and is not new in conception, only in the way in which the mechanical arrangements are carried out. It has as yet to be thoroughly tested by trial; but the experiment is an important and promising one, and the results will be awaited with interest, though some years must elapse before they can be arrived at.

Before leaving this instrument, which may

**Importance of Observations of the Moon's Place**

be taken as typical of modern changes in what has been called "meridian astronomy," I should like to explain why observations of the Moon's place among the stars have always been considered so important at Greenwich. Of the planets or "wanderers" which change their places among the stars, the Moon does so the most rapidly and consistently by far, because she is much the nearest to us and revolves round the Earth instead of round the Sun. The planet Mars, one of our next nearest neighbours, moves on the average more than twenty times as slowly among the stars: and so irregularly that at times he may be seen in nearly the same position for a fortnight or more, while in a fortnight the Moon regularly makes half the circuit of the heavens. The planets are not always so "stationary," but they do not move with anything approaching the speed or regularity of the Moon.

**Longitude at Sea**

Now any object in regular and determinate motion can be utilized as a clock. If we know where it will be at a given time, we can tell the time from its observed position; and to a sailor this is most important for determining his longitude, which may be regarded as the

time shown by a Greenwich clock when it is noon at the place where the sailor is—"noon at the ship," as it is called. The sailor can tell when it is noon at the ship by watching when the Sun reaches his highest point (or by an equivalent process); but it used to be a very difficult matter to find the Greenwich time at that moment, and thus infer how far he was east (or west) of Greenwich. Now-a-days he carries this information with him in the shape of a very accurate chronometer, which he sets to Greenwich time before he starts on his voyage, and which keeps to Greenwich time all the way, allowing for a certain "rate" which can easily be determined. But before the present century this was not possible, for no one knew how to make a clock or watch which was not altogether upset by changes of temperature; and the sailor found the determination of his longitude a considerable difficulty. He could readily determine how far north or south he was of his intended port, but not how far east or west. Sometimes he was fain to dispense with this knowledge entirely, and sail due south (or north) until he reached the proper latitude, and then sail due east (or west)

until his destination came in sight. Even then he was sometimes in such ignorance of his longitude that he believed himself to be due east when he really was due west, and sailed for days in the exactly wrong direction before discovering his mistake. The problem of "finding the longitude at sea" was a very serious one indeed; and the best chance of solving it seemed, in the seventeenth century, to be afforded by using the Moon as the clock to indicate Greenwich time.

**Foundation of Royal Observatory**

There were two difficulties to be reckoned with. The first, that though the Moon moves much faster than any other planet among the stars, its motion is still very slow, and to tell the time by it is like trying to read the exact time to a second from a clock with an hour-hand only. Still, even a rough knowledge of the Greenwich time would be useful to a sailor, and this difficulty was not vital. The second was far greater, viz., that at this time the motions of the Moon could not be predicted. Not only had no sufficiently good calculations been made to form tables of the Moon's future place, but no observations had been made as a starting point for the calcula-

tions. The first thing to do, obviously, was to start regular observation of the Moon, and for this expresss purpose the Royal Observatory at Greenwich was founded in 1676. The two centuries which have since elapsed have given us much information, but not by any means all that is wanted; it is still necessary to go on with these observations, and to improve them if possible. The object for which the Royal Observatory was founded is still rightly regarded as one of the main objects of its work. Airy considered it still the chief object, and he had a drastic way of bringing his opinion home to others. When, for instance, a luckless observer had some accident with an observation of the Moon, overslept himself, perhaps, on a cold morning, or otherwise lost an opportunity, Airy's rebuke would be conveyed in some such terms as these, written out on a piece of paper (as was his custom in all business transactions), and laid on the desk of the delinquent: "The Royal Observatory was founded for observation of the Moon. We get about 300 observations of the Moon during the year in all; and the Observatory costs the nation £6,000 a year. Hence each observation of the Moon is worth £20; and by

losing one last night you have cost the nation £20!" The effect of such a rebuke has been described as "prodigious."

**The large Equatorials at Greenwich**

It has been remarked that when Airy mounted a 13-inch telescope at Greenwich in the fifties, he did so almost apologetically, and in the earnest hope that its use should not interfere with the proper work of the Observatory, which was to be done with instruments distinguished for their steadiness rather than their size. Recently, however, the equipment of the Greenwich Observatory has been developed not only in a manner which Airy would have thoroughly approved, but also in the way of large equatorials. In the first place, Airy's 13-inch refracting equatorial has been replaced by one of more than double the aperture—a telescope with a lens 28 inches in diameter. The largest in the world[1] at the present moment is 40 inches in diameter, so that in this respect our national Observatory is some way behind the record; but there are only three larger refractors in existence than the Greenwich

[1] At the moment of writing, the lens for the Paris telescope is not yet made.

28-inch—one at St. Petersburg of 30 inches, and two in America of 36 and 40 inches, of which mention will presently be made. The 28-inch is economically attached to the old mounting built by Airy for the 13-inch already spoken of; it was originally intended to house it actually under the same old dome, or rather drum, though the telescope, being necessarily longer as well as larger in cross-section, would have in that case projected through the shutter opening. This arrangement was not one which any one would have chosen if he could help it; but it meant asking our Government for a small sum instead of a larger one, and the Government is not fond of giving large sums to science. Accordingly, the Astronomer Royal decided to put up with inconvenience so as to get his large telescope in any case, and to house it in the old drum. He might have successfully done so, but, fortunately, he was saved from having to make the attempt; for the old drum, which had stood thirty years' wear and weather in the interests of its old friend the 13-inch, struck work at the new proposal, and refused to rotate any longer. A new dome was necessary in any case; and though the tower

**Domes**

foundation was also rather small, by introducing a novelty in dome-shapes, Mr. Christie got a house large enough to contain his big telescope in all positions. The new dome is shaped rather like the head of a small mushroom, projecting beyond the tower, which forms its stalk, and it has the additional novel feature of opening by the sliding apart of two halves, instead of having a series of shutters. The work has been admirably carried out by Messrs. Cooke & Sons, of York, who were the first to suggest making domes of papier-mâché, which has allowed of a great reduction in their weight.

It may be asked why a new tower and mounting were not provided as well. The answer is that, not only was there a saving of expense, but also the old mounting for the 13-inch was known to be a good one. All Airy's mountings were firm and steady, and no better instance of this can be given than the fact that this particular mounting, designed to carry a telescope half the size, now carries the new 28-inch with the perfection of steadiness.

But this is not the only addition to the

**The Thompson Telescope**

Greenwich equipment in the shape of a large equatorial. Sir Henry Thompson has presented to the Observatory a large photographic refractor of 26 inches aperture, and attached to the same mounting a beautiful reflecting telescope of 30 inches aperture, made by Dr. Common, F.R.S. Not only is this addition noteworthy in itself, but it is a representative instance of the way in which many of the largest telescopes have come into the possession of astronomers, as gifts from rich men, and, I am glad to add, rich women. In such cases there is not the same need to study small economies, for these generous benefactors give with a free and open hand. The addition of the reflector to the refractor (which alone was originally contemplated) is a case in point. When it was seen that this addition would materially enhance the value of his gift, Sir Henry Thompson hastened to increase his original estimate, and the result is a magnificent instrument, forming a fitting crown to the new buildings at Greenwich.

**The photographic Instruments**

The mention of the Thompson twin-telescope brings us to the third head of the four under

which I have classed the recent developments at Greenwich—the use of photography. A beginning was made with the Kew photo-heliograph in 1873 for taking daily records of the Sun's surface; and it is to be remarked that the photographs were from the first, not taken and then stored away, but studied and measured at once, so that the "Greenwich Observations" are in this, as in other fundamental work, the standard place of reference for records of Sun-spots. A larger instrument for solar work has since been presented to the Observatory, also by Sir Henry Thompson.

In 1887 an International Conference, of which more will be said later, met at Paris, to consider the project of mapping the whole sky by photography; and one of its most important resolutions decided the pattern of instrument to be employed by the eighteen observatories which undertook to share the work. Of these our national Observatory was one, and an instrument of the standard pattern was soon afterwards erected at Greenwich. The total photographic equipment is thus considerable. It is not necessary to

describe it here in detail, as much of what will be said later of the general course of astronomical photography will apply to Greenwich.

**The Spectroscopes**

Nor is it necessary to say more of the spectroscopes which have been acquired, than that the Observatory is fully equipped for taking its share in that new department of astronomy which has been called astrophysics.

**The New Buildings**

It remains only to notice that all these new instruments have necessitated large additions to the buildings. Not only new domes for protecting the instruments have sprung up like mushrooms (one of them at least rather like a mushroom), but new libraries, computing-rooms, storehouses, and workshops have been added, so that the Octagon Tower, which was the whole Observatory at its foundation in 1676, and remained its chief architectural feature for two centuries, is now rivalled by a stately building of cruciform shape at the south end of the grounds; and the "Seven Domes of Greenwich," when seen from a distance, have

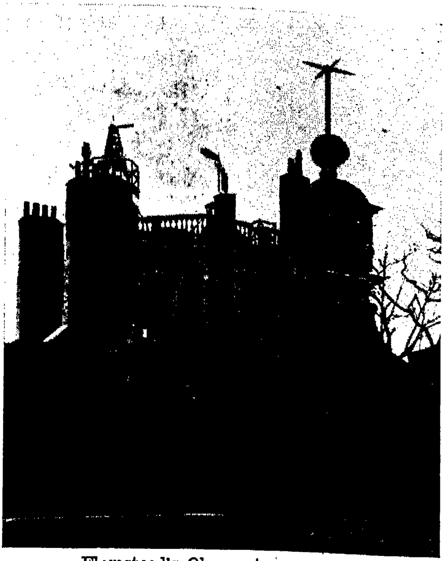
the appearance of a small astronomical city set on a hill.

**These Changes typical of Changes elsewhere**

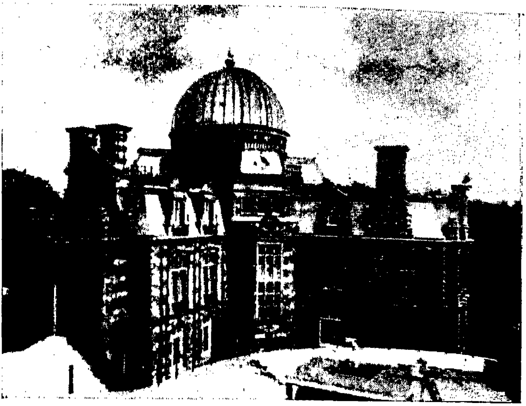
And now I shall endeavour to show that each of these lines of development of our national Observatory is thoroughly representative of what has been done elsewhere. In the astronomical world generally new instruments of precision have been erected or invented: telescopes have increased in size and number, new observatories have been established—private munificence having especially helped in this direction; photography has come to the aid of astronomy in numerous ways, and the spectroscope has founded a new science of its own.

**The Almucantar**

And, first, as regards instruments of precision. We saw that at Greenwich the transit-circle, the standard instrument for observing the places of the planets and fixed stars, has been supplemented by another instrument very nearly resembling it, but not restricted to use on the meridian. Elsewhere the lonely supremacy of the transit-circle has been more seriously attacked. I do not here dwell on Dr. Gill's use of the heliometer for determining planetary positions; for the helio-



Flamsteed's Observatory, 1676.



The new Physical Observatory, 1899.

THE ROYAL OBSERVATORY, GREENWICH.

meter is not a new instrument, and the novelty will be dealt with in the next section as a change in method. But an entirely new instrument has been suggested and used with success by Mr. S. C. Chandler, of Boston, U.S., to which he has given the name of Almucantar. It is essentially a telescope firmly fixed to a float, which can thus rotate round a vertical axis, but always remains accurately at the same angle with the vertical. The accuracy of the transit-circle is secured by restricting observations to a vertical circle of the sky, usually the meridian; the almucantar is similarly restricted to a horizontal circle. But there is this essential difference between the methods of securing the restriction: in the case of the transit-circle it is secured by human skill and labour in turning two pivots to an accurate shape. Each of the pivots of Airy's transit-circle took six weeks of unremitting and tedious toil[1] before its shape was perfect

[1] In a paragraph describing the celebration of Sir George Airy's ninetieth birthday (July 27th, 1891) occurs the following passage:—

"There were many other guests whose presence was significant, as for instance, Mr. Biddell, who was forty years ago charged by Messrs. Ransomes &

enough to satisfy the exacting astronomer.

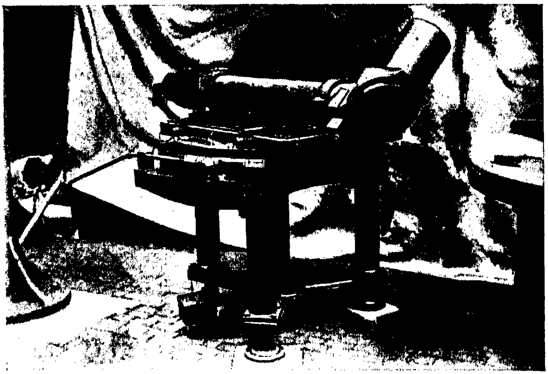
For the almucantar, on the other hand, no art or labour is required. The faithfulness with which it keeps to its prescribed path depends on the innate properties of floating bodies, and is thus liable to no human imperfections.

I have no hesitation in predicting a great future for some form of almucantar: for the instrument may be modified in various ways so long as the essential principle of flotation is retained. In one sense it is not a new instrument, for it is closely allied to the old floating collimator. Mr. Chandler cannot claim entire originality for his instrument any more than he can

May with the construction of the present transit-circle. He described to a small knot of most interested guests the dismay of the workmen and their employers at the demands of Sir G. B. Airy, especially those relating to the pivots. These were to be of chilled iron, six inches in diameter, and perfect cylinders to within $\frac{1}{30000}$ inch! No error of this magnitude was to be discernible with a delicate spirit-level; and after trying all the most delicate methods of turning then known, the requisite accuracy was obtained by sheer labour—rubbing down bit by bit all the places which this same spirit-level indicated as too high. Each of the pivots cost six weeks of such labour!"—*The Observatory*, vol. xiv. p. 291.

claim to have discovered the principles of floating; but he has at any rate shown us a practical working form of instrument, and made a valuable series of observations with it. Though he has himself ceased to observe with such an instrument, and though no one else has yet taken advantage of the opportunity

**The Durham Almucantar**



THE DURHAM ALMUCANTAR IN THE WORKSHOP.

to follow him, Professor Sampson, of Durham, is on the point of beginning work with an almucantar, which has been made by Messrs. Cooke & Sons, of York. In this instrument an ingenious alteration of Chandler's original design has been introduced at the suggestion of Dr. Common, F.R.S., the telescope being of

the "broken" kind, with a mirror at the elbow joint, so that the observer always looks in a horizontal direction. But the principle of any form of almucantar is the same—fix a telescope to a float so that it always remains at the same angle with the vertical; and the details can be varied in many ways. The telescope may be a photographic one for instance, and I shall show later how the photographic method can be used with either transit-circle or almucantar. The advantage of the latter, which may ultimately recommend it in preference to the transit-circle, is its independence of workmanship.

**Photometers** Among new instruments of precision due to the last quarter of a century, we may fairly include those for measuring the brightness of a heavenly body, instead of, or in addition to, its positions. It is curious how little had been done in this direction previously, for the study of changes in brightness is a most fascinating one. It was, for instance, the sudden appearance of a new bright star which attracted the attention of Tycho Brahé to astronomy; and the observation of variable stars has always been a department of the science in great

favour with astronomers. But they seem to have been reluctant to provide themselves with an instrument for accurately *measuring* the brightness; the usual plan has been to estimate differences of brightness between the variable and neighbouring stars, and it is only quite recently that photometers have been extensively used. Of such there are two classes, one depending on the equalization of two lights, the other on the extinction of light. In the first class a standard light, either of a particular star or of an artificial star, is varied by some device, in a measurable manner, until it is equal to the light of the star selected for examination. The device usually adopted depends on the properties of polarized light; but as a simple example we may suppose a wedge of neutral-tinted glass passed in front of the standard light, which is thus obscured more and more as there is a greater thickness of glass to pass through, until it is found equal to the star under examination. The wedge should be graduated in the direction of increasing thickness, so that the reading of its position, when the two lights are judged equal, may be registered as a measure of the brightness of the selected star.

**Equalization**

If this is originally brighter than the standard we must reverse the process and pass the wedge gradually in front of the former until it is diminished to agreement with the standard. Such a wedge, however, has hitherto been exclusively used as the second kind of photometer, which depends on the principle of

**Extinction**

extinction. If the range of thickness be great enough, then as the wedge is gradually passed in front of a star the light becomes dimmer and dimmer, until at last the eye fails to perceive it at all. The reading at which this "extinction" occurs varies with the brightness of the star, and is therefore a measure of the brightness; and we get rid of the necessity for a comparison star to a large extent—not entirely, for in our variable climate we must make allowance for the state of the sky, and it is advisable therefore to make constant measures of some star of known brightness to determine this allowance; but in each separate observation we are concerned with only one star instead of two, and this is an advantage which recommends the "wedge photometer" as an extinction photometer. It was introduced by the late Professor Pritchard, in 1882. In the following few years his assistants at

Oxford accomplished a remarkable piece of work in cataloguing the brightness of nearly 3,000 stars, and for this work he obtained in 1885 the gold medal of the Royal Astronomical Society jointly with Professor E. C. Pickering, of Harvard University Observatory, who had commenced a similar though larger work in 1879 with an equalization photometer. Pickering has largely added to this since 1885, and a fine research has also been carried out by Müller & Kempf at Potsdam with a Zöllner photometer, which is of the equalization class. The instruments of Zöllner, Pickering, and Pritchard are the three chief photometers at present in existence, but there are also photographic methods of measuring the brightness of the stars of which mention will presently be made.

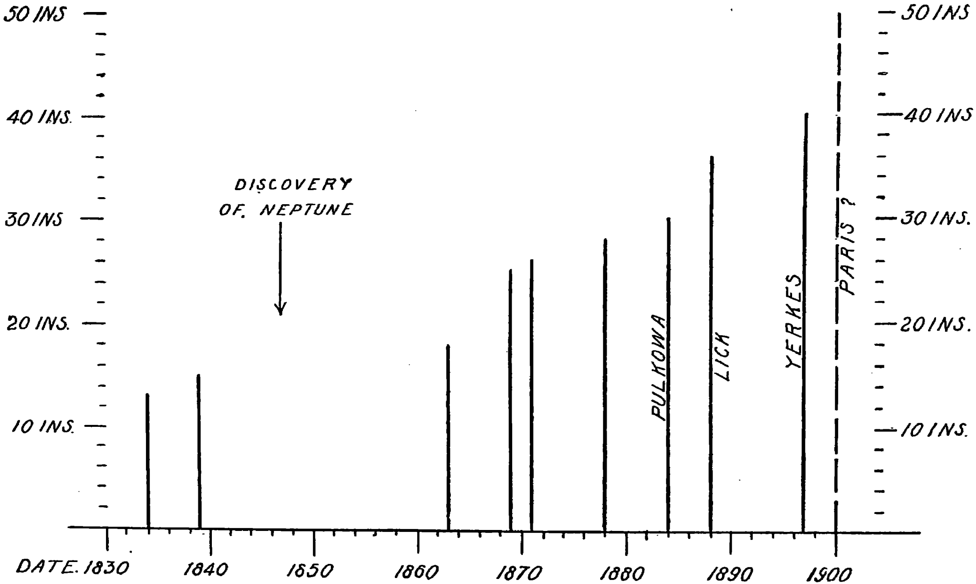
**Large Refracting Telescopes**

Passing now to the development of large telescopes, we find that the 28-inch lens of Greenwich, large as it is, has been considerably surpassed in America by the 36-inch lens at the Lick Observatory, in California, and the giant 40-inch lens at Yerkes Observatory, near Chicago. Indeed, nowhere is the difference between the third and fourth quarters of our

century more clearly indicated than in the history of large refracting telescopes. In the diagram is shown the size of the largest telescope (of this kind) in existence at different dates; and the periods of relative inactivity in the middle of the century, and of sudden development recently are both plainly marked. The date of the discovery of Neptune is indicated on the diagram; for it might be supposed that a great event like this would have a stimulating effect on almost all branches of astronomy; but it will be seen that no such influence is discernible. The size of telescope is indicated in the usual way by the aperture of the lens in inches, and not by the length of the tube. It should be remarked that a telescope of over 50 inches aperture is being constructed by that thoroughly able optician, M. Gautier, of Paris; but although the tube is erected in the Paris Exposition, at the time of writing (August, 1900), the lens is not made, and hence the line is dotted in the diagram.

**Reflecting Telescopes**

Before saying anything in detail about these fine instruments, it may be remarked that there is another kind of telescope altogether (not included in this diagram), in which a




APERTURE OF THE "LARGEST TELESCOPE IN THE WORLD" AT SUCCESSIVE DATES.

concave mirror takes the place of the lens. This form has several distinct advantages over the other; in the first place, all the colours forming white light can be brought accurately to the same focus, whereas with a lens the focussing can only be approximate. Even the approximation is only secured by combining two or three lenses together, and a different combination is required according to the use to which the telescope is to be put—whether for gazing with the eye, or for taking photographs. A refracting telescope suitable for visual work cannot be used for photography, and *vice versâ*, whereas a reflecting telescope can be used indifferently for both purposes. In the second place, a reflecting telescope has only one surface to be made optically perfect, whereas a compound lens has at least four. Thirdly, the surface, and not the substance, is of chief importance. The light does not penetrate the substance of the mirror, and thus, so long as the surface is perfect the interior may be faulty. The lens, on the other hand, must not only have its four surfaces without flaw, but the substance of the glass as well. For all these reasons it is much easier to make a large reflector than a refractor of the same size, and

the largest telescopes in the world are reflectors, and do not appear in the above table of refracting telescopes. The largest is still Lord Rosse's giant reflector, with a 6-foot reflector (a mirror 6 feet in diameter), at Parsonstown, in Ireland; but the mirror being of speculum metal is not so highly reflective as the modern silver on glass, and this instrument is therefore not so effective as Dr. Common's 5-foot reflector, made by himself and mounted in his garden at Ealing, which may be regarded as effectively the most powerful instrument in the world.

But the size of a reflecting telescope has not been regarded with the same interest as that of a refractor—why, it is rather difficult to say. The latter is in many respects a handier and more trustworthy instrument, the former being subject to curious moods which render it at times difficult to work satisfactorily. (Dr. Common has been heard to compare the reflector to the female sex for uncertainty.) But this disadvantage is more than counterbalanced by the advantages above mentioned. Perhaps the difficulty of making a lens has invested the refractor

with a special interest. It has several times been suggested that the limit of what is practicable in the size of lenses has been reached, and thus there is a special kind of record-breaking in making a larger one. Whatever the reason, there is no doubt that when a large telescope is mentioned it generally means a refractor; and American millionaires and other generous benefactors to astronomy have usually spent their money on refractors such as those in the above list.

**The Pulkowa Telescope**

The three largest are worthy of special notice. First comes the 30-inch erected at Pulkowa by the Czar of Russia. About 1835 the Czar Nicolas determined to have the finest observatory in the world, and he instructed the famous Wilhelm Struve to see that it was built, giving him practically *carte blanche* as regards expense. It was built at Pulkowa, twelve miles from St. Petersburg, and a straight road leads from the observatory to the city—so straight that a telescope placed in the central tower of the observatory can look into the market-place. The buildings and equipment are magnificent, and the observatory contained a large refracting

telescope for that epoch—aperture 15 inches. When all was finished (in 1839) the Czar came to inspect it, and, after being shown over the observatory, turned to the director and asked simply whether he was satisfied; to which the diplomatic astronomer replied that he was—*for the moment*.[1] And excellent use he made of the noble observatory and instruments. But in course of time telescopes of greater size were built elsewhere, and the 15-inch was no longer in the front rank. So that after forty years the Czar Alexander III. instructed Wilhelm Struve's son and successor, Otto Struve, to provide him the largest refractor in the world; and in consequence the 30-inch Pulkowa telescope (the joint work of the Alvan Clarks, of Washington, who made the lens, and the Repsolds, of Hamburg, who made the mounting) was erected in 1884.

**The Lick Telescope**

It did not, however, long enjoy its proud supremacy. The Czar of all the Russias was outbidden twice successively by American millionaires. First by James Lick, whose

[1] This story was told me by Otto Struve at Pulkowa in 1887. The actual words used by him were: "Struve, sind Sie zufrieden?" "Augenblicklich."

bones lie beneath the great 36-inch on Mount Hamilton, in California. The establishment of the Lick Observatory, with its wonderful equipment and climate, may be classed among fortunate accidents. The story goes that Mr. Lick, having acquired considerable wealth, was desirous of erecting a permanent memorial of himself and his wife; and his first idea took the form of two immense statues on the Pacific coast, which should be a landmark visible for a considerable distance. About this time, however, an enthusiastic astronomer suggested the much better plan of building a giant telescope as a mausoleum. He adroitly pointed out that in case of war landmarks of the sort contemplated would be liable to bombardment by the enemy; whereas a telescope high up on Mount Hamilton would be quite safe, and the instrument would be kept in good repair, and the memory of its founder ever fresh, by the devoted astronomers. This excellent advice was taken, and though the munificent benefactor of astronomy did not live to see his monument completed, he died in the happy assurance that his bones would ultimately be interred under the largest telescope in the world, placed in the finest situa-

tion as yet selected for an observatory. There is reason to believe that the Pyramids were both astronomical observatories and the tombs of Pharaohs. In modern times James Lick was fired with the same ambition as the Pharaohs, and realized it with something of the same magnificence. By choosing the site for the observatory with judgment, the telescope, large as it is, has been rendered more effective still to an extent which it is difficult so overestimate; and it will be recognised that the expense was at the same time considerably increased, for a good road had to be made to the top of a mountain 4,250 feet high, and all the materials taken up. It was well worth doing. A large telescope in a poor climate is useless, for the rays of light which fall on different parts of the large lens are differently disturbed by atmospheric tremors, and produce a confused image when combined at the focus. When the air is not steady a small telescope gives better images than a large one, and is much easier to handle. But there is no question about the excellence of the climate on Mount Hamilton,—"God's own climate," as one of the observers has reverently described it,—and the legacy of James

Lick has a double claim on the gratitude of astronomers. It should, however, be remembered that to those in actual charge of the telescope the situation is not without its disadvantages. They are at some distance from the nearest town, and without many of the comforts of civilization. The winter on the mountain is severe, and brings with it at times considerable privations. In one winter there was actually no water to drink except what had passed through the engines.

In spite of such discomforts, and of some faults in administration which robbed the observatory of some of its ablest observers, the little colony on Mount Hamilton has worked cheerfully and enthusiastically from the first, and thoroughly justified expectations.

**The Yerkes Telescope**

It is to be hoped that the discomforts of the Lick observers will diminish as civilization creeps round the base of Mount Hamilton. The value of land in the neighbourhood began to go up when the telescope was erected, so that very soon the telescope and observatory, costly as they were, could have been built out of the profits on land sales. So I have heard

on good authority; and further (though this has been contradicted), that to this circumstance is due ultimately the existence of the Yerkes telescope, which is four inches larger than the Lick. For some enterprising gentlemen in another neighbourhood, desiring to test the generality of the law that if a large telescope were built the value of land in the neighbourhood would go up, announced a still larger telescope, and ordered two 40-inch discs of glass for the lens. The experiment succeeded admirably, and they were so well satisfied with the rise in price which followed on the mere announcement (so the story goes) that they considered it unnecessary to proceed further with the experiment. Be this as it may, the fact is undoubted that two beautiful 40-inch discs were produced to order, not ultimately claimed, and being left on the maker's hands were to be had comparatively cheap.

No one was, however, able to take advantage of the unique opportunity, until another millionaire, Mr. Yerkes, of Chicago, came to the rescue. Two eminent astronomers of the city called upon him one day and explained the situation, and suggested that it would be

a fine chance for Chicago. He responded in the most generous manner, and the construction of the telescope was undertaken at once by Alvan G. Clark, the only remaining member of the firm which had made the Pulkowa and Lick object-glasses—three times called upon to beat their own previous record. The complete instrument was exhibited at the Chicago World's Fair in 1893, and is now mounted in a splendid observatory at Williams Bay, about eighty miles from Chicago. Here it is in the hands of three men of world-wide fame: G. E. Hale, director of the observatory, who has perfected a new line of research in the study of the Sun's surface; S. W. Burnham, the first authority on Double Stars, and E. E. Barnard, who discovered the fifth satellite of Jupiter. Burnham and Barnard were both formerly at the Lick Observatory, and are thus able to compare the performances of these two giant telescopes. As yet they have made no invidious declaration in favour of either; and such a decision would, no doubt, be difficult, for while advantage of size is in favour of the Yerkes, that of climate is with the Lick. The climatic advantage even extends to material comforts, for severe as are

the winters at Mount Hamilton, it seems that those at Williams Bay are severer still. A temperature of 40° below zero is not unknown, and though the recent winter (1899–1900) was described as mild, the lake was frozen over in March to a depth of fifteen inches!

**The Bruce Telescope**

To these great endowments by Americans there are many smaller ones to be added; and notably those by an American lady, the late Miss Catherine W. Bruce, of New York. Her benefactions have been numerous, and (although something of a digression) one especially is worthy of notice, because of its felicitous form. Instead of giving some immense gift to one particular observatory, she had the happy idea of offering a number of small sums of about £100 to astronomers in any part of the world, to supply any wants which they might be otherwise unable to satisfy. The scheme was a great success; it brought to light a number of pressing needs which were delaying good work for the want of comparatively small sums, and both the generous donor and Professor E. C. Pickering, who organized the distribution of the gifts (and who probably originated the idea), must have been immensely gratified by the result.

But this is, as above remarked, a digression. The special gift to astronomy which introduces the name of Miss Bruce here is her presentation to the Harvard University Observatory of the largest photographic doublet in existence—with a lens made up of *four* single lenses, like a portrait lens. The telescope is thus in a different category from the Lick and Yerkes telescopes, but being the largest in existence of its own class, may be mentioned alongside them. It also resembles the Lick telescope in having the advantage of a first-rate climate; for Professor Pickering, instead of keeping it at Harvard (where the climate is by no means bad, but not specially good), has sent it to the branch observatory at Arequipa, in Peru. The very existence of this branch establishment is due to another benefaction; for Mr. Boyden left a sum of money for expeditions and experiments to determine the most suitable climate for astronomical observations, and the station at Arequipa is the outcome of the investigation. Until recently, far too little attention has been paid to this vital matter of climate. Observatories have been built in or near large towns from force of circumstances quite independent of astronomi-

cal considerations, and it is only at this latter end of the nineteenth century that attempts have been made, as in the case of the Mount Hamilton and Arequipa Observatories, to get the inestimable advantage of a good climate for observation.

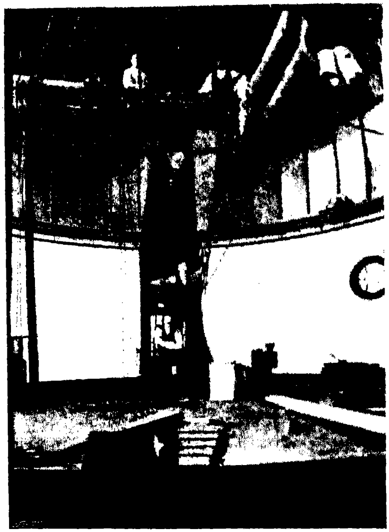
**The McClean Telescope**

Passing from America to our own country, it is pleasant to note that, although a little behind the United States in the matter of millionaires, we in England have also our munificent benefactors. I have already mentioned Sir Henry Thompson's generosity to our Royal Observatory at Greenwich, and I may now remind you that one of the Visitors of the Royal Institution [1] has, besides founding the Isaac Newton Studentships at the University of Cambridge, which have already done much for mathematical astronomy, recently presented a pair of large telescopes to the Royal Observatory at the Cape of Good Hope, with apparatus providing for visual, photographic, and spectroscopic work. From these fine instruments, in the able hands of Sir David Gill, we may look for important results in the near future.

[1] This book is an expansion of three lectures delivered at the Royal Institution.

**Rising Floors**

Before leaving the subject of large telescopes, I would call attention to one new departure which their recent great development in size has necessitated, viz., the rising-floor. When



SETTING UP THE McCLEAN TELESCOPE AT THE CAPE OF GOOD HOPE.

using a small telescope the observer can accommodate himself to its different positions by using a small step ladder. But as the instrument increases in size, the dimensions of this ladder become inconvenient, and finally

impossible. To meet the new requirements, Sir Howard Grubb, the well-known astronomical instrument maker in Dublin, hit upon the idea of a moveable floor, which should rise and fall, in the manner of a lift or elevator, by machinery under the electric control of the observer. The device has been applied to the Lick and Yerkes telescopes; to the Washington 26-inch; to the telescope presented by Mr. McClean to the Cape; and to several others, and with conspicuous success: but it is not without considerable attendant dangers. When the rising-floor of the Yerkes telescope had just been erected, owing to faulty engineering[1] it slipped from its fastening wire-ropes, and fell the whole distance from the top of its range to the bottom. Most fortunately there was no one on the floor at the time, but several people had only just left it, and photographs of the wreckage make one shudder to think of what might have happened. There is no doubt that the accident was due to negligence, and that it is easy to make the device thoroughly safe; and we may

[1] I should make it clear that though the idea of the rising-floor originated with Sir H. Grubb, he was not responsible for the workmanship of this particular floor in any way whatever.

hope that this serious warning has lessened the possibility of any such accident in the future.

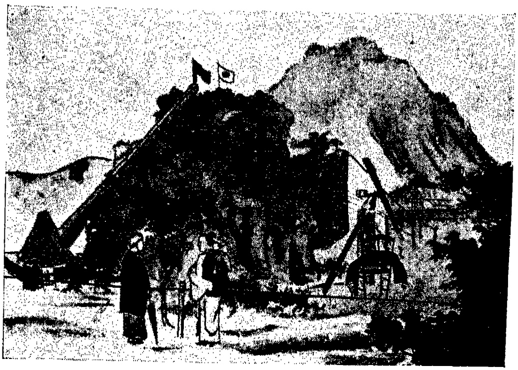
**The Gelatine Dry-plate**

We now come to the most important new weapon with which astronomy has been provided since the invention of the telescope—the gelatine dry-plate. It may seem strange to particularize in favour of the dry-plate over photography generally; many people are doubtless accustomed to regard the dry-plate as a mere gain in convenience of manipulation. But in astronomy the dry-plate is all-important; it removed a limitation under which photography previously suffered. With a wet-plate, a photograph could not be exposed for more than a short time; the film dried, and ceased to be sensitive. With the advent of the dry-plate came not only a gain in sensitiveness, but the possibility of prolonging exposures indefinitely. For they are not limited by the duration of a single night; when dawn or cloud comes, it is only necessary to put the cap on the telescope and wait for the next fine night, when the exposure may be resumed. Plates have been left in the telescope for weeks or even months. Hence the very

faintest objects can be photographed—and it is a common experience now to photograph objects too faint to be *seen* in the largest telescope.

**The Camera of the Astronomer**

The camera of the astronomer does not differ in essentials from that which takes



AN ASTRONOMER'S LONG CAMERA
(40 feet).
*Drawn by a Japanese Artist.*

your portrait; indeed, much astronomical work is now being done with portrait lenses. But to get a picture on a large scale, he must have a *long* camera, and he uses one as long as a telescope. [In the illustration is shown a camera 40 feet long used by Professor Schae-

berle for the eclipse of 1896 in Japan. He found a rock placed very fortunately to act as a support. The drawing was made for me on the spot by a Japanese artist.]

His instrument is indeed usually called a photographic telescope, though this is somewhat of a misnomer; for a "telescope," an instrument for seeing things at a distance, is a combination of object-glass (or mirror) and eye-piece, connected by the tube; and for photography we remove the eye-piece and substitute a photographic plate. There is no need, however, to be very critical of nomenclature; and the name photographic telescope has the advantage of reminding us that the instrument may be either a refractor or a reflector, as in the case of the visual telescope. In the latter case a concave mirror is used to form the image—in the former a lens or series of lenses. The special form of refractor in which two compound lenses are arranged as in a portrait lens is generally distinguished as a photographic doublet, and it is a large instrument of this kind that Miss Bruce gave to the Harvard Observatory. Such instruments as are being used for the International Chart of

the Heavens have only one compound lens, and the name photographic refractor is usually applied to these only.

**Relative Advantages of Refractors, Reflectors, and Doublets**

All three forms of instrument have their special advantages and disadvantages. The reflector has the great advantage that since light is not analysed into colours by reflection, the images formed have absolutely no colour fringes at all, whereas with any form of lens there is some colour, though the skill of the optician may reduce its effects nearly to the vanishing point. But the reflector has only a very limited field; if we attempt to photograph with it the stars scattered over a considerable area of the sky, we shall find that only within a small circle of less than a degree in radius are the images even approximately round; outside this they become elongated, and the images are useless for purposes of refined measurement. The field of the refractor is rather larger, though with an ordinary object-glass it is not possible to extend it very far. Mr. Dennis Taylor, of Messrs. Cooke & Sons, York, has lately invented a form of triple object-glass, which constitutes a great advance in this direction. When, how-

ever, we come to the portrait-lens or doublet, the field is greatly extended. Instead of one or two degrees' radius we can get ten or even twenty. So great is the difference that the portrait-lens has hitherto been regarded with some mistrust; it is feared that the image of such a large field cannot be accurate because other instruments, known to be accurate, are so strictly limited in field. But much of this mistrust is ill-founded. We have recently been measuring at Oxford some plates kindly lent by Professor Pickering, of Harvard, who has championed the portrait-lens from the first, and we find them wonderfully accurate. We find that although a certain allowance must be made for what is called "optical distortion," yet the law of this distortion is so simple and well-defined (it varies as the cube of the distance from the centre of the plate) that there is no difficulty in measuring the position of any trail on the plate to a degree of accuracy determined only by the size of the grains of the photographic film. I make this statement with some little reserve, for the investigation is so new that it is still proceeding; but I also make it with considerable confidence, and I venture to predict for the portrait-

lens, as for the almucantar, an important place in the astronomy of the future.

**How Photographs are taken**

To those readers who are unfamiliar with astronomical work, it may not be obvious how photographs are taken. One fact deeply impressed on the minds of those accustomed to have their portraits taken is that there must be a good light; if daylight is not strong enough (as happens in the winter), magnesium or electric light is used. By what light, then, are the stars photographed at night? The answer, "By their own light" will no doubt come as a surprise to many, but a few moments' reflection will show the difference between the two cases. A human being gives out no light of his own—he must be illuminated to be photographed; and if he is to be photographed *quickly* the illumination must be bright. Given patience on the part of operator and sitter, and the latter could be photographed by candlelight or moonlight — perhaps, even by starlight; but it would take a long time, and the operation is one which all concerned are usually glad to get over quickly. Now, with the heavenly bodies, such illumination as is possible is already provided; and we can use

no other. Astronomers would be only too glad to illuminate some of the objects a little more strongly if they could, so as to get the operation of photographing them over more quickly, but no searchlight that we could flash on a heavenly body would add appreciably to its illumination. We cannot get beyond the light sent us from the sky, and hence we must make up our minds for a long sitting. Otherwise the operation is much the same as is already familiar to those who take portraits or have them taken.

**Clock-work for following the Stars**

There is, however, one important respect in which the camera of the observatory differs from that of the studio. When taking a portrait, the photographer can bid his subject remain still, with more or less success. There is record of a man who commanded the Sun to stand still, but he is no longer available; the Sun and stars refuse to modify their courses for the camera. The alternative is for the camera to follow them in their movements, and good clock-work machinery is accordingly devised to secure this following of the stars with wonderful exactness. Recently this machinery has been perfected by what is

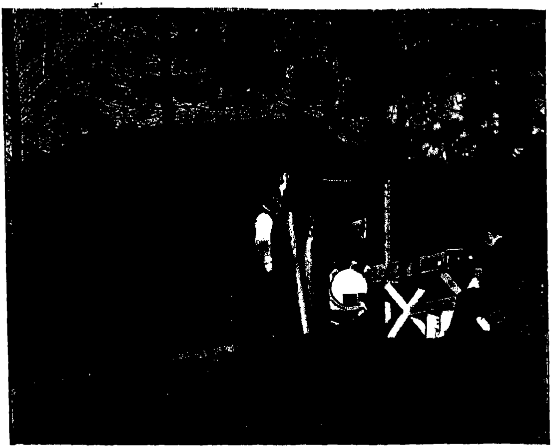
called electrical control, of which there are two forms. In one case the photographer holds in his hand two buttons connected with electric circuits, by pressing one of which he can accelerate the machinery, by the other retard it. While the plate is being exposed in one telescope, he looks through another telescope furnished with cross wires rigidly attached to the former, and keeps some selected star on the cross wires. Should it show a tendency to move in either direction, he presses the appropriate button and checks this tendency. Such watching is very tiring when continued for long; and so another form of "control" has been devised, in which a good clock plays the part of the watcher. As the clock pendulum swings to and fro it expects to find a certain state of matters in an electric circuit at each beat, which will mean that the machinery is going at the proper pace. Any failure to find this state of things sets up an electric current which amends the pace in the right direction. In this way there is but little difficulty in making the largest camera follow the stars with great nicety, so that the photograph is taken with as much ease as though they could actually be bidden to stand still.

**The Coelostat**

But an instrument has been recently brought into use which would have interested Joshua not a little; for although it cannot actually do what he did, it has the appearance of doing much more. The name Coelostat, which has been given to it, may be new to you (it only dates from 1895), though you may have heard of heliostat and siderostat. A siderostat is a mirror rotated by machinery so that the reflection of a particular star remains fixed in direction. Any one looking into this mirror at the image of this star would imagine it stationary, though stars near it would be seen to revolve slowly round it. The heliostat is essentially the same instrument put to the slightly different use of counteracting the Sun's motion. Looking into its mirror the Sun would appear to stand still, though imperfectly; for he would be all the time twisting round his centre as though impatient under the restraint. Now the coelostat is an instrument which makes the whole sky appear to stand still. Looking into its mirror we should imagine, not merely the Sun's centre, but all his disc to be stationary; not merely one star but all the stars, to be reduced to rest. For photographic purposes this is clearly a great

gain. The idea of the instrument is simple, and was expounded long ago by a Frenchman called August; but its advantages have been little recognised until recently. It has been used with great success on the British eclipse expeditions. Beautiful 16-inch plane mirrors for these instruments were made by Dr. Common, whose experience of grinding mirrors, acquired in making his own 5-foot telescope, already mentioned, has been so often placed at the service of others. These mirrors are mounted with their plane surfaces parallel to the Earth's axis, and are made to rotate round an axis parallel to the Earth's axis once in forty-eight hours—no other machinery or adjustment is necessary. Looking into a mirror which fulfils these conditions, a star seen in any direction will remain in that apparent direction until the mirror has turned so far round that the star is lost at the edge of the mirror. Thus the photographer can point his camera at the image of the sky in the mirror, just as though it were a fixed object—fixed, that is, with reference to the Earth. Hence the camera can be fixed, all the necessary movement being supplied by the mirror; and this is a great convenience when the camera is 100 feet long,

such as was used in America to observe the last eclipse of the Sun (May 28th, 1900). The illustration shows the first coelostat made in England for eclipse work, being set up for trial in the garden of the University Observatory at Oxford. The mirror was made by Dr.



THE COELOSTAT.

Common, and the instrument designed by him. The camera pointed to it is a double one, two tubes side by side, each with its own lens and its own image. It is pointed at the mirror so as to receive the reflected images of the Sun, which may be seen upon the ground glass.

The observer is prepared to test the going of the instrument by seeing whether these images remain stationary—whether the Sun really "stands still."

Measuring Plates

When a celestial photograph is taken, the astronomer's work is not over; it is rather only beginning. He has only brought down into his study a little bit of the sky for examination; but the examination is still to be performed, though it is done more conveniently in the study than at the telescope. Suppose, for instance, that he is concerned with a "double star," a pair of stars revolving round each other in a time and manner which it is required to determine. Before the days of photography he measured the relative positions of these stars with a micrometer at the eye-end of the telescope. Looking into the eyepiece he sees not only the two star images, but two parallel spider-lines, which can be rotated like the hands of a watch to any position-angle, the angle being read off on a circle graduated like a watch dial but more elaborately. One of the observations to be made consists in setting the spider-lines parallel to the line joining the stars (which is

The Telescope Micrometer

best done by making one of them actually pass through the star-images), and then reading the graduated circle. In this way it is observed that the line of junction points successively, as years roll on, to all the figures on the watch-dial, and so the revolution of one star round the other is proved, and the time of revolution found. For a complete description of the motion, however, we must observe also the *distance* between the stars, which is similarly subject to variation. This is the use of the second spider-line. The distance between the spider-lines can be varied in a measurable manner by a screw very carefully made; the head of the screw is expanded into a large circle and graduated round its rim, so that fractions of a whole turn can be read off with exactness; and hence, if the spider-lines be set at such a distance apart that one falls on the image of one star, and the other on the other, we have the means of measuring the distance of the stars from each other,—not, of course, the actual distance in millions or billions of miles, of which we may know nothing, but the angle subtended at the Earth, which varies proportionally to it.

These observations of "position-angle and distance" with a micrometer at the eye-end of a large telescope play an important part in astronomy. Not only are the movements of double stars followed in this way, but those of satellites, especially faint satellites; and the places of small planets and comets are found by measuring their position-angles and distances from neighbouring stars.

**Photographic Micrometers**

Now, when photography comes to the aid of the astronomer, it does not make these measurements for him. He can take two photographs of a pair of stars at different dates, and see by inspection of the photographs that they have changed their relative positions, just as we may see that a person has grown older by comparison of two portraits taken at different dates. But in astronomy we want far more than this general knowledge, and we cannot evade the necessity for accurate measurement. The photographs must still be submitted to the micrometer, though this is no longer at the eye end of the telescope, but placed conveniently in the study. More important still is the advantage that the measures can be made at any time when once the

photograph is secured; they can be repeated if any mistake is suspected, and repeated many times, and by several persons, for the elimination of accidental and systematic errors. We gain immensely by taking the photograph, but we do not avoid the necessity for a micrometer.

[To prevent misunderstanding, it should be remarked here that photography has not as yet rendered much service in double-star work. The most interesting pairs are so close together that their images on the photograph are generally inseparable, and the measurement must be done by eye at the telescope; but the example given sufficiently illustrates a general principle.]

There are many forms of micrometer for measuring plates. The most obvious form closely resembles that already described for use on the telescope; for measures can be made precisely in the same way on the photographed images of stars as on the images themselves. But to take this instrument as it stands is to lose some of the opportunities opened up to us by photography, which we are only gradually realizing.

**The Réseau**

One most important addition to the micrometer since it has been used on photographs is the *réseau*, a network of lines forming small squares impressed on the plate by a second exposure, independent of that to the sky, and developed along with the star images. Instead of measuring the position of one star with respect to another, we measure the positions of all stars with reference to this *réseau*; and this is found to be an immense gain in convenience and accuracy. For instance, if two stars are very far apart we should have to turn the micrometer screw a large number of times in order to measure the distance between them; and not only does this take time, but errors are apt to creep in when a long screw is used. With a *réseau* on the plate, each star is referred to the cross lines of the network in its immediate neighbourhood, which only requires a small portion of screw; and the relation of the different parts of the *réseau* is determined once for all.

**Rectangular Co-ordinates**

As a consequence of the introduction of this *réseau*, the method of measuring position-angles and distances has been almost given up; and there has been substituted the measure-

ment of "rectangular co-ordinates," that is, of two distances at right angles, instead of an angle and a distance. The method may be compared with that by which a person finds his way in American cities, where the streets cross at right angles and at nearly uniform distances apart. He knows how many "blocks" to pass going eastward, and how many "blocks" to pass going north; and when he comes to the particular block containing the person he wants, there is a further subdivision into houses and rooms which guides him. On a star photograph the streets are the cross lines of the *réseau*, and the "blocks" are the squares formed by them. The place of a star in a particular block is all that need be measured by the micrometer, and the simple knowledge of the place of the block completes the desired information. The subdivision of the "block" may be conducted by a micrometer screw as before; or better by two screws at right angles. But there is one form of micrometer in which screws are dispensed with, and there is instead in the eyepiece a small scale of equal parts, on which the observer reads the distances. He thus gains considerably in rapidity; for to screw backwards and forwards

takes an appreciable time, which is saved by the use of a scale; and although with a fine screw greater accuracy can undoubtedly be secured than with a scale, it has been found that even steel screws are liable to serious wear, which necessitates constant examination and application of troublesome corrections to the readings, whereas a scale does not, of course, alter in the same way.

**Photographic Photometers**

Suppose again that the brightnesses of the stars are under examination. A photograph shows at once which are the brightest by the size of their images. This size has nothing whatever to do with the actual size of the bodies which send us the light—they are so far off that all the light comes as if from a single point—it only represents the imperfections of our instruments. A star image[1] ought to be a mere point of light in the largest telescope, but for a variety of reasons (firstly, because the lens, however large, is finite in area, and this introduces what are called "diffraction" phenomena; secondly, because no combination of

[1] The case of the planets must be carefully distinguished from that of the fixed stars. The planets show sensible discs, which increase in size as a higher magnifying power is used.

lenses can focus all the colours making up white light accurately to the same point; and thirdly, because human workmanship is not perfect, and even the focussing of one colour is only approximate) the image only approximates to this form, and though there is a central point of light it is surrounded by a circular patch, which rapidly decreases in brightness and is not bounded by any well-defined edge, but merely becomes too faint to be further perceived; and in a star photograph bright stars give larger images than faint ones, because more of this patch leaves a record on the plate. We can get a very fair measure of the brightness of the star by measuring the size of the image; but the indefinite character of the edge introduces difficulties; for instance, different persons would make quite different measures. And again, images of the faintest stars shown on the photograph do not differ in *size*, but only in blackness (or greyness). Thus even with star-images it is better to measure the *density* of the image in some way rather than its size; and when we pass to pictures of the planets, or of the Sun's corona, the density of deposit is the only guide to the brightness of the source of light. The true photographic

photometer should thus measure the density of deposit in some way; and one of the simplest instruments for doing so is Sir W. Abney's revolving sector, an instrument of beautiful simplicity. The principle is as follows:—If from an opaque circular disc we cut out a piece of sector form, and then rotate the disc round its centre in a beam of light, the beam is sometimes stopped by the remainder of the disc, sometimes allowed to pass through the sector cut away; so that a screen placed in the beam is sometimes illuminated and sometimes not. When the rotation of the disc is slow, these alternations of light and darkness can be recognised separately; but when the rapidity of rotation is increased, there comes a time when the eye entirely fails to notice the alternations, and the effect is that of a steady illumination of the screen by a beam of less intensity than before. The apparent intensity is simply proportional to the angular opening of the sector. If then we have side by side two beams of light of equal intensity forming patches of light on a screen of equal brilliance, and if we obstruct one of them by the photograph whose density we wish to measure, and the other by the revolving sector, we can obtain a measure

**Abney's Sector**

of the density by varying the aperture of the sector until its obstruction is precisely equal to that of the photograph. By an ingenious device this variation of aperture can be effected while the sector is in rapid rotation; and even the angular opening read off without stopping the rotation. This instrument is a most valuable addition to the astronomer's equipment, though its uses are by no means confined to astronomy.

**The Spectroscope**

It has been remarked that the spectroscope has already created a new department of astronomy, so vast that it is practically a separate science, and has been called Astrophysics. The spectroscope can, however, scarcely be regarded at this date as a modern astronomical instrument. It is as much the instrument of the chemist and physicist as of the astronomer. The general principle of analysing the light received from a body into its constituent colours, and thereby recognising the nature of the source of light, is by this time probably quite familiar to those whose scientific knowledge is of the slightest; and instrumental details are comparatively unimportant. Still, to give a

proper idea of astronomical methods of work, and of discoveries with the spectroscope which will follow later, it will be advisable to describe briefly the arrangements of the instruments used in astronomy.

**The Objective Prism**

The simplest form of spectroscope, but one with limited applications, is a simple glass prism, such as used to be hung from a chandelier. Most children of a generation ago, and some of the present, have seen with delight the patches of "rainbow" thrown on the walls by the Sun shining through such prisms. These patches are, however, not of much use for scientific observation, for they represent the confusion of overlapping images. If there were seven distinct and exact colours, red, orange, yellow, etc., as in the ordinary language, then there would be seven distinct and exact images of the Sun in these colours; but even then there would be confusion, owing to the overlap of the images. As a matter of fact, the colours are by no means separate and exact, but have every gradation: there are millions of colours, and so millions of overlapping images, and great confusion.

If, however, we look at a star through the

prism, there is no such confusion, for the images are points of light which do not overlap; and the total result is a thin line of light of different colours. We may use a telescope to magnify the effect, and we then have one of the forms of astronomical spectroscope, a prism with a telescope behind it. The prism is of course much larger and more accurately made than one from a glass chandelier; it must be as large in area as the lens of the telescope behind it, and is a good deal thicker than the lens at its thickest part. A prism of this kind is therefore a costly thing, but its work is well worth the cost. It is essentially the spectroscope of the astronomer, for chemists and physicists generally deal with flames and other sources of light, which are not points, but require limitation by a slit; it is only the astronomer who finds in the stars, and on occasions of an eclipse, sources of light which are already points and lines, and thus require no artificial limitation. Moreover, this form of "objective prism," as it is called, has been hitherto chiefly used by the astronomer who is also a photographer, and wishes to photograph the spectra of the stars (or of the disappearing crescent

of the Sun at an eclipse), especially if he wishes to make a comprehensive survey. If a telescope be pointed to the heavens with a photographic plate placed at the focus, then the images of a number of stars will fall on the plate and be photographed as points at wide distances from each other. But if the prism be placed in front of the object glass, each little star image becomes a line instead of a point, so that we get by one exposure the photographic spectra of a number of stars, whereas with the slit-spectroscope we can only get one at a time—that of the particular star on the slit.

**Giving Width to the Spectrum**

Before passing to the slit-spectroscope, we may remark how a defect already noticed in star spectra (viz., that the spectrum is only a line of light, difficult to read because of its narrowness) may be removed when dealing with it photographically. Instead of using a cylindrical lens (which answers the purpose of giving width to the spectrum, but is apt to introduce other errors), we may allow the star to vary its place on the plate slightly during the exposure. Such variation must not take place in the direction of the length

of the spectrum, or we shall introduce just that "confusion" noticed at first, but in the perpendicular direction, the effect of which is thus to repeat the spectrum above or below its previous position, and so give it a sensible width. This method can also be used with the slit-spectroscope, which is arranged as follows:—

**The Slit Spectroscope**

The light from the Sun or other heavenly body is allowed now to pass first through the telescope and form an image at the focus. Of this image all but a line of light is there stopped by a screen, the line of light passes through the slit in the screen, which must be very carefully made, with edges as straight and parallel as possible. Its width can be varied according to requirements.

Now this light cannot be passed through a prism as it is, for it is diverging from the focus to which the telescope made it converge. A collimating lens makes the rays parallel, as they were before entering the telescope, and we are then ready to use an apparatus exactly similar to the former (viz., a prism and a telescope), though of smaller dimensions, so that the prism can be relatively thicker at its

base; or we may have two or three prisms, without running to enormous expense. In this way we can get much greater dispersion in the spectrum, *i.e.*, separate the different colours more effectively, so that the slit-spectroscope, besides being the only possible form of instrument for objects of sensible area like the Sun, is also the instrument for work requiring the greatest accuracy.

**Gratings**

It should be remarked that in both the slit-spectroscope and the objective prism form, we may substitute for the prism a "grating"—a series of lines ruled close together with the greatest nicety, many thousands of them to the inch. Such an apparatus analyses white light in the same way as a prism, but with special advantages over the prism, and also some disadvantages. Generally speaking, the spectrum is more regular but fainter with a grating.[1]

[1] It may be added that a good grating is also much more difficult to get than a prism. The best gratings are made under the direction of Professor Rowland, of the Johns Hopkins University, and each one takes some months to make. The ruling is done by a diamond-point on silver, the spacing between the lines being effected by a screw moved by machinery. Needless to

**The Spectro-Heliograph**

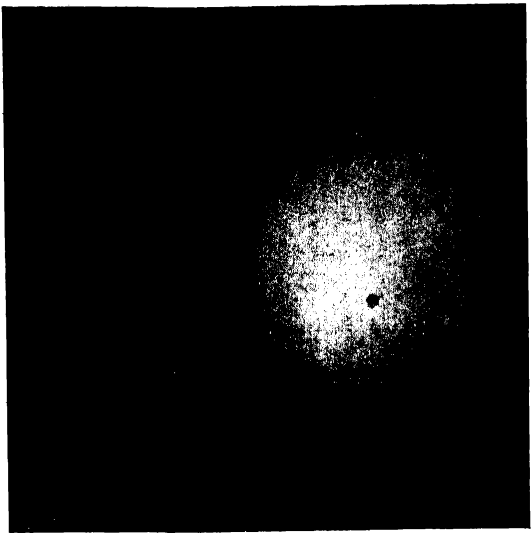
There is one form of the slit-spectroscope which is as essentially astronomical as the objective-prism, and is called the spectroheliograph. It is used to photograph parts of the Sun's surface, which in the ordinary way are lost in the surrounding glare, but owing to their peculiar light can be made to reveal themselves under appropriate conditions. The ordinary light of the Sun when displayed as a spectrum is found to be "continuous," *i.e.*, there is light of all colours, except for the dark Frauenhofer lines. There are parts of the Sun,

say, the screw must be of an accuracy which tests the maker's art to the utmost; indeed, an accuracy is required which the maker cannot attain, and his workmanship is supplemented by an ingenious device. When the screw has been made it is carefully tested by refined measurements capable of detecting its errors, and these are all noted. It is then possible to say, for a given position of the screw, how much the diamond-point is in advance of or behind its proper position; and apparatus is arranged to correct the error. Those who have the opportunity of seeing this beautiful mechanism at work at the Johns Hopkins University (in Baltimore) should not neglect it, but it may be well to tell the following little story by way of caution. It should be premised that one of the main difficulties in making a grating is to find a suitable diamond-point, which can only be done by trial. The experienced workman in charge of the

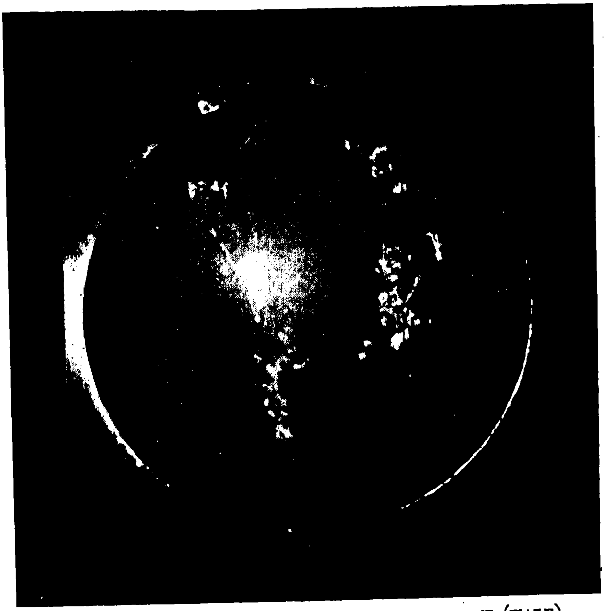
however, which do not give such a spectrum, but one of only one or two colours. Such are the "red flames" seen at the edge of the Sun's disc on the occasion of a total eclipse: the spectrum of these consists of a few bright lines, one of which is a brilliant red line. Now, though the light of these flames is faint compared with the total light of the ordinary surface, which is made up of so many colours, if we arrange to stop out all colours but this particular red, we diminish enormously the brightness of the continuous spectrum, without affecting that of the red flames appreciably;

machine turns a diamond over and over until he finds a likely-looking point, but on trial it turns out unsatisfactory. It is a matter of "luck"; he must go on trying until he finds one, and sometimes it is months before the trials prove satisfactory. Now on one occasion he had had a particularly long series of disappointments, and had at last succeeded, and an important grating was about half-ruled. A party of visitors were admitted to see the beautiful machine and duly admired it, but unfortunately the party included one who had been there before, and who was so anxious to show his familiarity with these matters that he pointed out the "thing that was doing the ruling" with his finger, touching it gently as he spoke. Alas! the gentlest touch was sufficient to set the poor workman again looking for a good diamond-point.

and in this way we can actually make the red flames appear brighter than the rest of the surface. We could stop out the other colours in a rough sort of way by looking through red coloured glass, but this is not sufficiently accurate for our purpose. We must use a spectroscope to separate all the colours into a band : then put a slit so as to admit the exact red we want only, and stop all the rest : and on this principle the spectro-heliograph is constructed. Other parts of the Sun, besides the red flames, or chromosphere, which can be photographed in this way are the so called "faculæ," bright patches seen generally in the neighbourhoood of spots near the edge of the Sun's disc. When a photograph of the Sun is taken in the ordinary way (see illustration), faculæ are *only* seen near the edge : for in the middle of the disc they are lost in the glare of the ordinary light. With the spectro-heliograph, however, they can be photographed all over the Sun's disc, as will be seen in the illustration given. In this case it is not red light that is used but two lines near the violet end of the spectrum. This beautiful instrument has been used with great success by its inventor, Professor G. E. Hale, now Director of



AN ORDINARY PHOTOGRAPH OF THE SUN,
TAKEN AT GREENWICH.



THE SUN AS SHEWN BY THE SPECTRO-HELIOGRAPH (HALE).

the Yerkes Observatory. I believe it was during a trip to Europe, and after a chat with Sir William Huggins, that he thought of the instrument, and promptly returned to the United States to put his idea into practice. It rewarded him by complete success, and it is to be hoped that when the new Yerkes Observatory is completed, which is now nearly the case, we shall have from it as regular a record of the prominences and faculæ as is obtained of the spots at Greenwich and elsewhere.

**Concluding Remarks.**

Such, then, are some of the new instruments which have come into our hands quite recently for the exploration of the heavens. They are so numerous that the description of each has necessarily been scanty, and I fear the general effect of the enumeration may be confusing. The director of a large observatory of many departments, where it is necessary to use most or all of them, may well sigh for the old days of simplicity, when he would have been fully equipped with a transit-circle and an equatorial. But for those who can limit their attention to one or two, the variety for choice is splendid. There are instruments to suit all

tastes, and, it may be added, all pockets. Those who feel, as Sir William Huggins tells us he felt in 1859, dissatisfied with the routine character of ordinary astronomical work, may follow him into the new science of Astrophysics. An ordinary man cannot buy a telescope like the Yerkes 40-inch; but this large telescope must clearly be devoted to work which his 4-inch or 6-inch cannot reach, and therefore leaves a clear field for him in departments which he can work at. Or, if he has no telescope at all, he can for a few pounds get a micrometer or photometer, which will enable him to do first-rate work in the measurement of photographs; and there are vast stores of photographs already taken awaiting measurement. It is only necessary to make an earnest beginning, and work and means will find themselves.

# Section II

## MODERN METHODS

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## MODERN METHODS

In the preceding section we have taken stock of the various new instruments recently put into the hands of astronomers. Most of these have necessitated quite new methods of work, which we now proceed to consider. But before doing so, we may notice one or two "modern methods" which are the natural outcome of progress and experience, and would have been adopted if these new instruments had not come into being. A conspicuous example is the change in attitude towards the old problem of finding the Sun's distance.

**Transits of Venus**

Thirty years ago, although the Sun's distance was known to be between ninety and ninety-five millions of miles, the actual figure was in doubt by some millions of miles. But two transits of Venus were in prospect (the transits of 1874 and 1882), and it was hoped

that by observations of these the margin might be reduced to one-tenth of its width. The hope was terribly earnest: it was scarcely realized that the method itself might break down, and leave us no better informed as to the Sun's distance than we were before; but there was of course always the chance of bad weather at so many of the stations that the opportunity would practically be lost, not to come again for over a century (for transits of Venus occur in pairs at long intervals, and the next pair will not come till the twenty-first century). It is recorded of Mrs. Somerville, who was able to carry on scientific work after she had passed her ninetieth year, that one of her chief regrets in dying was that she should not "live to see the distance of the Earth from the Sun determined by the transit of Venus, and the source of the most renowned of rivers, the discovery of which will immortalize the name of Dr. Livingstone." Those who have already attained middle life will remember the excitement caused by the transits of Venus in 1874 and 1882. Observers who had been most carefully drilled beforehand were sent out to all quarters of the globe to make the requisite observations, and

the result was awaited with almost breathless expectation. It was a great disappointment. Unforeseen difficulties robbed the observations of their expected accuracy, and though the margin of our uncertainty as to the Sun's distance was reduced, the reduction was trivial compared with what had been hoped for. The next pair of transits of Venus will occur in 2004 and 2012, but it is doubtful whether they will attract much attention, for we no longer look to these opportunities as the best for determining the Sun's distance. The excitement of 1874 has left the name "Transit of Venus" ringing in the ears of the present generation, but henceforward the name will probably only be familiar to astronomers.

**Sun's Distance from Observations of Mars**

For not only has this method of determining the Sun's distance failed, but another one has already attained a large measure of success, and is likely to be more successful still. We know very precisely the *relative* distances in the solar system from one of Kepler's laws, which originates with the law of gravitation itself. This law connects the distances of the planets from the Sun with the times in which they revolve round the Sun; and

these are known with extreme accuracy from comparison of observations made long ago with those of modern times. We can therefore make a most accurate map of the solar system at any moment, leaving unknown just one thing, the *scale* of the map; and if we know any one length in miles, this supplies the scale and therefore all the other lengths. Hence it is not necessary to measure the actual distance of the Sun; that of a planet will do equally well, and the best distance to choose for actual measurement is the shortest; just as we can judge with our two eyes of the distance of near objects, though we find it more difficult for distant ones: indeed, for objects so far away as the heavenly bodies we entirely lose our perception of relative distance, and the Moon seems as far away as the stars.

**'arallax** The method of gauging distances in astronomy closely resembles that by which we estimate them with two eyes. To look at a near object with both eyes we must draw the pupils together, closer and closer as the object approaches us. If it is brought very close the convergence of the pupils becomes ugly and painful, and is called a "squint"; but the

squint is only an extreme form of what happens in all cases—our muscles turn the pupils, or the axes of the two eyes, so that both point to the object, and the muscular effort varies with the distance of the object, and so gives us a notion of that distance. When the muscular effort is entirely relaxed, the eyes have the well-known appearance of gazing "into vacancy," or at an object at an infinite distance.

This apparatus which we carry in our heads for gauging distances is copied on a larger scale by the astronomer. For two eyes at a couple of inches apart, he substitutes two telescopes as far apart as he can get them, which on our earth is something under 8,000 miles; since we increase the power of our apparatus directly in proportion to the length of this "base," as it is called, the distance apart of the two stations from which observations are made. (If our eyes were two feet apart instead of two inches, we should be able to gauge distances twelve times as well.) For the muscular effort in turning the eyes to convergence on the object, the astronomer substitutes the reading of graduated circles, or

some other method of telling how much the telescopes are inclined to each other. This is the quantity he wishes to measure, the "parallax," as it is called, being simply the angle between the directions in which the telescopes are pointed. The angle is zero when the object is at an infinite distance, the telescopes then being parallel, and it increases with the proximity of the object. Since it is easier to measure a large angle within a given percentage of error, than a small one, the astronomer chooses for observation the nearest object he can get, because its parallax is largest. Until recently the nearest suitable object for determining in this way the distance of the Sun was the planet Mars.

**Gill's Expedition to Ascension**

In 1877, therefore, Dr. (now Sir David) Gill took a heliometer to the lone island of Ascension, and spent six months there determining the distance of Mars. In the first chapter I ventured to specify the year 1875 as something of an epoch in astronomical history, from which new developments in various directions may be dated. There is nothing beyond curious coincidence to establish; but as a curious coincidence we may remark that the value of

transits of Venus practically ended with 1874, and within a few years the modern method of determining the Sun's distance was initiated.

Unlike the projects for observation of the transit of Venus, which began so hopefully and ended in disappointment, Gill's expedition began with disaster and ended successfully. The instrument he was to take with him to Ascension was constructed for European latitudes, and as he thought it possible that when set up for a tropical latitude it might not work well, he set it up for trial in the rooms of the Royal Astronomical Society some little time before starting. His doubts proved only too well founded. Scarcely had the instrument been erected as it would be in Ascension, when it overbalanced and came crashing down on its delicate eye end, practically a wreck. The curious may see to this day a bruise on the table in the meeting-room of the Society, which is a reminiscence of this disaster. But Sir David Gill is a man not lightly discouraged; he got all the instrument-makers in England to lend their aid, and the instrument was repaired (impossible as it seemed immediately after the accident) in time for him to start as

he had intended. He arrived at Ascension without further mishap, and then other troubles began: there were thick clouds to prevent his seeing Mars, and his health was not good. But he moved his point of observation and got beyond the cloud-bank, and he and his devoted wife eventually triumphed over other difficulties. The whole story is vividly told in Lady Gill's *Six Months in Ascension*.[1] Here we can only add that the expedition was thoroughly successful in its main object; and not only was the margin of uncertainty in the Sun's distance considerably reduced, but the observations called attention to other matters of importance, as will presently appear.

**The Diurnal Method**

One point has not been made clear. In describing the method of gauging the distance of Mars, reference was made to a *pair* of telescopes placed at the ends of the base, and Sir David Gill only took with him one instrument, his heliometer. The fact is, one telescope can be made to serve as a pair if observations be made both morning and evening, for the rota-

[1] *Six Months in Ascension.* An unscientific account of a Scientific Expedition. 8vo, London, 1878.

tion of the Earth carries the instrument round continually to different places, and we can thus get a succession of observations from different points, which, though not made simultaneously, can readily be treated as though they were simultaneous. We have said that our perception of the relative distances of objects in ordinary life is largely due to our possessing a pair of eyes, and that a man with only one eye loses this perception in a marked degree. But this is because his head remains steady. If his head rotated as the Earth does, and he directed his one eye persistently towards the same object so long as it remained in view, he would be able to gauge its distance by the apparent change of direction as the eye passed across, a change which would be large for near objects, and small for distant ones. The idea of a rotating head is perhaps too uncomfortable for sensitive nerves; but the principle can be sufficiently illustrated by merely twisting the head from side to side as far as it will go, keeping one eye closed. It will be seen at once how near objects change their places relatively to distant ones; and precisely in the same way a single telescope can observe the change of direction,

and hence the parallax, of Mars or another planet with reference to the stars, and thus serve the purpose of a pair.

**Defect of Mars Observations**

The observations of Mars in 1877, though thoroughly successful, were made under one disadvantage. In measuring the angular distance of Mars from neighbouring stars, Sir David Gill was comparing a disc of light with points of light; and, moreover, the disc was of a different colour from the stars, being of a reddish tinge. Any one familiar with observational or experimental work will recognise that such differences as these are liable to introduce small systematic errors. When, for instance, the ordinary balance is used to weigh any substance, the weights in one scale usually differ in size and shape from the substance to be weighed, and we must apply a small correction for the different buoyancy of the air. We can estimate this with considerable exactness, but we may not be able to gauge the effect of air currents, or something else depending on the difference of shape and size which may escape attention. We must be prepared, in fact, for minute errors which would not exist if the two scales were filled exactly alike,

though we may not be able to assign the cause of these errors very definitely. And so in other cases: it is generally better to have things which are to be compared in any one particular as nearly as possible the same in other particulars, for we can often see how differences may affect our result; and even if we cannot see this at the time, there is no harm in guarding against the possibility that we may have overlooked some way in which an apparently irrelevant difference may act. In the observations of the place of Mars among the stars, the dissimilarity of the planet and the stars was an undoubted disadvantage; but this disadvantage disappears if a minor planet be taken instead of Mars, for a minor planet is so small as to present no sensible disc—it is strictly comparable with a star in fact. On the other hand, the minor planets are all considerably farther away than Mars, with an important exception to be presently mentioned; and so we lose something by taking one of these instead of Mars. Nevertheless, Sir David Gill found it advantageous to do so, and, with the help of several other astronomers, he determined the Sun's distance three times more from observations of the minor

**Parallax from Observations of Minor Planets**

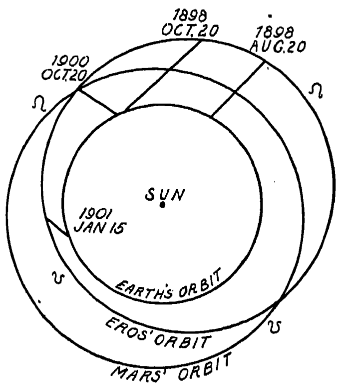
planets Victoria, Iris, and Sappho, getting results on all three occasions beautifully in accord, and much more accurate than that derived from Mars. These three results undoubtedly constitute a great advance on anything that has ever been done. Combining the results in the best way, Sir David Gill finds for the Sun's distance

92,874,000 miles.

To show the close accordance of the observations, it may be stated that the observations of Victoria alone give a result greater than this by only 7,000 miles. Sappho alone indicates 40,000 miles less, and Iris alone 100,000 miles greater. [The Mars observations in 1877 indicated a distance 200,000 miles greater; but, in Sir David Gill's own words, "the accuracy of the *Mars* observations is very inferior to that realized in the observations of the minor planet *Victoria* in 1889."[1]] Thus, while the long-expected transits of Venus left us uncertainty to the extent of perhaps a million miles, Sir David Gill's observations have reduced this margin to within 100,000 miles.

[1] *Mon. Not.*, *R.A.S.*, vol. liv. p. 344.

Striking as is this advance, we are probably on the eve of a still further step, owing to the recent discovery (in 1898) of the minor planet Eros. This is the exceptional minor planet above referred to which comes nearer the Earth than Mars. By its occasional near approaches to the Earth, it presents new opportunities for **Eros**




ORBIT OF EROS.

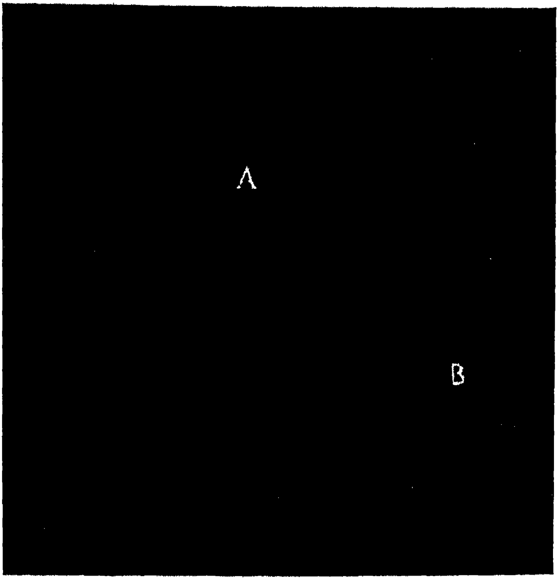
determining the Sun's distance, one of which has come upon us while this book is in press. During the present winter (1900–1) astronomers are as busy as they were at the transits of Venus, with the same object and nearly a hundred times better chances of success. There may not be so much stir to reach the

ears of the public; there will be no expeditions for instance; photographic telescopes are sufficiently scattered over the Earth's surface, without moving them from the fixed observatories where they regularly dwell. Nor is any very startling result to be expected as the outcome of the work: the adopted value of the Sun's distance cannot be much in error, though if the small correction required can be obtained with certainty, it is a matter of the first importance to astronomy. But though the occasion may yield nothing sensational to the general public, it is of exceptional interest to astronomers, who are hard at work photographing the planet on every fine night.

A photograph taken at the University Observatory, Oxford, on October 12th, 1900, is here reproduced. Each star is shown nine times over, the plate being exposed nine separate times in slightly different positions —five of them soon after sunset, and four of them just before sunrise. Under the letter B are seen the nine separate images of a pair of stars, which maintain their relative positions throughout. Under the letter A are the five evening images of Eros and a star,

and it will be seen how Eros moves away from the star.

The uppermost pair of images are close together, but the lowest pair is considerably




A PHOTOGRAPH OF EROS
(Taken at the University Observatory, Oxford, on October 12th, 1900).

separated, Eros being above the star. The morning four images of the star are shewn to the right of these five, but the planet had by that time moved quite out of the picture.

The lines crossing the picture are the *réseau* lines, which have several times been mentioned.

There will not be so favourable an opportunity again for thirty years, unless another unexpected discovery of a small planet should be made. Thus the history of the last quarter of a century, in reference to this great problem of the Sun's distance, may be summed up as follows:—Our period opens with the failure of the method of transits of Venus, which had been looked forward to with such eagerness and confidence for half a century at least. But almost immediately the modern method was employed with a fair measure of success on the planet Mars. Much better results were soon after obtained from three minor planets, Victoria, Iris, and Sappho (all this work chiefly by Sir David Gill, though others heartily co-operated with him); and within a few months a still more favourable opportunity will probably give us the best knowledge of the Sun's distance we are likely to get for the *next* quarter of a century.

**Heliometer Observations of the**

In explaining this change of attitude with regard to the method for attacking this

**Planets generally**

problem, something has necessarily been said of the results obtained, anticipating perhaps what would naturally come in Section III. But we now turn to a "modern method" of which the results cannot yet be quoted because they belong to the future. The method was suggested by Sir David Gill, as an outcome of the work which has just been described. It was thereby made manifest with what wonderful accuracy the positions of the planets among the stars could be determined—an accuracy far greater than can be attained with the transit-circle. Commenting upon the observations of Mars, Professor Simon Newcomb (the greatest living authority on such a matter) remarks that "a minute inequality, which would never have been noticed in even the best meridian observations (*i.e.*, observations with the transit-circle) was brought to light, and mapped in a diagram so as to be unmistakeable." And the observations of the minor planets were more accurate still. Hence Sir David Gill puts the pertinent question: why should a method capable of this accuracy not be used more generally instead of only on special occasions? Has not the time come when the transit-circle observations of the

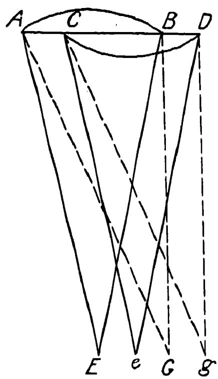
planets should be superseded by observations with the heliometer?

**The Heliometer**

The heliometer is not a new instrument. It might quite reasonably have been added by Airy to the Greenwich equipment; but Airy had little sympathy with equatorial work, and the heliometer is a form of equatorial, if we include the micrometer at the eye end. The use of a micrometer has already been described: it will measure the angular distance between a pair of stars and the direction in which they are separated. The heliometer does exactly this work in a more complete and efficient manner than the micrometer. Speaking generally, we may say that the heliometer can measure points further apart than the micrometer. Its principle of construction is as follows:—

The image of a star at the focus of a telescope is formed by light coming from all portions of the lens. If we cover up a part of the lens the image remains in the same place, but is less bright, because we have taken away part of the light which went to form it. It is also slightly different in shape, the "diffraction phenomena" being affected by the shape of the lens, which is now no longer circular; but these

effects we will neglect for the present. If, then, an object-glass be cut in two along a diameter, each half may be considered as forming its own image, the two images being superposed to form one only so long as the two halves of the glass are in their usual position. But now,




PRINCIPLE OF THE HELIOMETER.

suppose we slide them asunder in the direction of the division, each half will carry with it its own image, and we shall get two images of a star, separated by just the interval separating the halves of the object-glass. Suppose now that there are two stars, E and G, in the field of view, and that we have rotated the object-glass so that its divided diameter is

parallel to the line EG, and let EeGg be the pairs of images formed by the divided object-glass. As we separate the halves further and further the distances Ee and Gg increase in exactly the same way; and there comes a position when one image *e* of one star coincides with one image G of the other. The separation is then exactly equal to the distance between the star images. We can measure this separation with great nicety, and so get the information we want. The position-angle of the line EG is got by reading on a graduated circle the position of the divided diameter which allows us to make this coincidence of images; for it can only be made when the divided diameter is exactly parallel to the line joining the stars.

Thus the heliometer does what the micrometer does, only in a better way. To explain the superiority completely would involve some rather technical matters; but two advantages can be stated with tolerable simplicity. Firstly, the observations with the heliometer are less dependent on the good going of the driving clock; for if two star images are made to coincide they will remain

coincident even if the clock drives badly. With the micrometer, on the other hand, the observer has to put one of the wires on one star and then to attend to the other star with the other wire: meanwhile, bad clock-driving may have upset his careful adjustment on the first star. Secondly, as already remarked, the heliometer can measure larger distances; because the separation of the lenses is watched by a *scale* instead of a screw; and the scale has several advantages—for one thing it is not liable to wear like a screw which is much used.

**Comparison with Transit-Circle**

But it is with the advantages of the heliometer over the transit-circle with which we are chiefly concerned for the moment, rather than its advantages over the ordinary micrometer, and the chief difference is this:—The transit-circle can only make an observation at a particular time, viz., when the object is on the meridian. We can, on the other hand, with the heliometer go on all night (or as long as the planet is visible) measuring its position with respect to neighbouring stars. It is true that we must, in order to get all the information we require, proceed then to find the places of these particular

stars on the celestial sphere: that is, we must make transit-circle observations of *them*, and at first sight it might seem as though we had gained nothing, but rather lost: for instead of one planet we have now several stars to observe with the transit-circle (one comparison star not being regarded as sufficient, for reasons which may be passed over for the present). But the difference is this: the stars are practically fixed in the sky, their movements during years, or even centuries, being very small; and hence we can accumulate observations of them at leisure. But the place of a moving planet is constantly changing, and an observation missed at one time cannot be compensated by another subsequently.

Hence Sir David Gill has proposed that we should enter upon a new method of planetary observation. Instead of merely observing the planets when they come to the meridian with the transit-circle, he proposes to make a number of measures throughout the night with a heliometer, of the place of the planet among the neighbouring stars, and then determine the places of these stars with the

transit-circle. The change may be illustrated from everyday life. Suppose we had a watch whose going we wished to test. One way of doing so would be to wait till the church clock struck at each hour and see what the watch read. Sometimes we might miss an hour by accident, but we should get a fairly good idea of the going of the watch even if it went somewhat irregularly. It might gain in the morning for instance, and lose in the afternoon; or go much faster in summer than in winter: all this we could find out. But suppose it went irregularly *in between* the hours: we should perhaps be unable to notice this from the hourly comparisons with the church clock. If, however, we had an intermediary in the shape of a clock known to be really good, which could be compared with the watch at any time, we could make our knowledge of the watch's going as complete and accurate as we pleased, checking the general rate of the good clock by the striking hours as before.

In this illustration the striking of the church clock at fixed times represents transit-circle observations limited to occasions when the

planet is on the meridian. Our erratic watch is a planet, whose general going we may observe by transit-circle observations, but about which we may obtain much more and better information by comparing it with the neighbouring stars—our intermediary good clock. We know that the stars go regularly, and their movements are sufficiently checked by the intermittent observations with the transit-circle.

**New Planetary Tables**

This proposal is of considerable importance. It is made at a most appropriate time: for not only are we on the eve of a new century, but our knowledge of the planetary movements has just been put on a new footing by the formation of new tables of the planets. The basis of any set of tables is the accumulated knowledge at the date of construction. Starting from this, tables are made predicting the places of the planets for the future. The comparison of these predictions with observations gives materials for correcting the tables. Such corrections are not made until a considerable mass of new observations has been accumulated since the last edition: then the great work is undertaken of discussing and co-ordinating all

these observations, made in different countries, with essentially different instruments, and by different observers, and of deducing from them the best possible revision of the old tables. This task is not only very laborious, but requires mathematical skill of the highest order; and the astronomical world owes a great debt of gratitude to Simon Newcomb, of Washington, for having just completed such a revision of the tables, which will enable them to start the new century with a new and closer approximation to the truth. Sir David Gill has already put his proposal for doing justice to the occasion into practice at the Cape Observatory, and there can be but one opinion as to the value of his work. The only question seems to be whether the photographic method might not give sufficiently good results with less labour. This is as yet to be tried; but in any case the principle remains the same: observations of the planets will almost certainly be made more often and more accurately than before.

**Photometric Observations of Jupiter's Satellites**

A principle of the same kind has been adopted by Professor E. C. Pickering, of Harvard University Observatory, in the observation of

the eclipses of Jupiter's satellites. To watch these eclipses is one of the simplest and most interesting observations which a beginner, equipped with a small telescope, can make. Jupiter, with his line of four moons, like little beads on an invisible string, can be seen in a very small telescope. At times which are given in the *Nautical Almanac*, one of them is seen to fade and ultimately disappear; it has passed into the shadow of the planet and lost the Sun's illumination. The moment when it is thus eclipsed is an accurate indication of its place in its orbit round Jupiter, and the observation is independent of any measuring apparatus. Such observations have therefore been regarded as giving us the best information we can get for calculating the orbits of the satellites; and from their simplicity and acknowledged value they have been made in considerable quantities. But they are found to be disappointingly inaccurate. The moment of disappearance depends a good deal on the observer, and certainly on the instrument employed, and there is a large "probable error" of observation. Professor Pickering has carried into practice a method of observing these eclipses which

gives very much more accurate results than the old method. He measures with a photometer the brightness of the eclipsed satellite compared with one of the others. Before it passes into the shadow the ratio of brightnesses is constant, but when it enters the shadow the ratio begins to diminish, and if observations are made (say) every ten seconds, the resulting ratios show a steady progression towards zero, which may be exhibited in the form of a curve or diagram. It is clear that by observing a large number of points on this curve instead of one only (the vanishing point), we enormously increase the accuracy of observation. Suppose, for instance, that a light cloud gradually passed in front of Jupiter near the critical moment. The observer using the old-fashioned method might lose sight of the almost eclipsed satellite without noticing (from the slightly diminished light of Jupiter) that the disappearance was due to cloud. But in the new way previous observations would be a guide to an accidental circumstance of this kind: cloud would obscure both satellites to the same extent, and leave the ratio little affected, or if it blotted out the fainter altogether would indicate an

abrupt termination to the curve which the rest of it would show to be accidental. A large number of these observations, extending over many years, are now in the hands of Professor Sampson, of Durham, who hopes to get from them material for new tables of Jupiter's satellites, though it will take a considerable time to do this.

**Measures of Saturn's Satellites**

There is another new plan of satellite observation which is certainly modern in the sense that it was first used recently, but there is no particular reason why it should not have been adopted long ago. It was initiated by Dr. Hermann Struve in observing the places of the satellites of Saturn with the large telescopes of the Pulkowa Observatory.

The change consisted in this: previous observers had measured the distance of each satellite from the body of the planet Saturn, and observed the position-angle of this distance. Now Saturn is a very bright object with a large disc, and a satellite is a faint point of light; and it is very difficult to make measures of two such dissimilar objects without considerable bias in one direction or the other—

a bias which is different for different observers and not easy to determine. Dr. Struve, therefore, gave up this method, and measured the position of one satellite with reference to another. Since both were moving round the planet, the changes of position became more complex, depending on the motions of both, instead of on that of one alone as in the old method. But Dr. Struve showed himself fully equal to dealing with the more complex calculations, and found several new and remarkable relations between the movements of the satellites, which former observations had been quite unable to detect. I well remember showing his early results to the late Professor Adams, who found them so astonishingly accurate that he could scarcely believe them. The work which followed showed, however, that there had been no mistake, and the whole investigation, which has been quite recently published, has put our knowledge of the Saturnian system on a new basis. This increased activity in a department of astronomy which belongs essentially to the old times is, perhaps, an even better indication of the new life which has been poured into the science than some of the essentially new

enterprises. There is no particular reason why the "triangulation" of Saturn's satellites, as this new method of measurement is called, or of Jupiter's satellites, or the photometric observation of the eclipses of the latter, should not have been commenced much earlier in the century: except that astronomers had settled down into a groove, and regarded novelties with some disfavour. They have been a long time in recognising properly the uses of photography, but having been once roused, they are readier to accept changes such as those just mentioned, which might have been made long ago.

**Changes of Structure in large Telescopes**

If we keep to the classification adopted in the first section, our attention is next claimed by changes in method which the increase in size of telescopes has suggested or necessitated. The most noteworthy recent improvements are mainly concerned with the comfort of the observer, which, though in some aspects a mere detail, is of the greatest importance in delicate observation, when the nerves and muscles should be as free from strain as possible. Sir Howard Grubb's invention of the rising-floor has already been mentioned.

This does a good deal towards keeping the position of the observer the same relatively to his instrument.

**The Equatorial Coudé**

M. Loewy, however, now director of the Paris Observatory, has done this much more completely with his equatorial *coudé*, though this involves a radical change in the form of the instrument. The observer sits in an arm-chair in a comfortably warmed room much as a man sits with a microscope. He looks in a fixed direction down the polar axis of the instrument, and the light from the star he wants to see is reflected to him by an arrangement of mirrors or prisms, passing through the proper lenses, of course, on its way. The parts of the telescope which follow the stars are out in the open air. He can direct them as he pleases by mechanism, close to his hand, of a very simple character. The actual shape of the instrument and its movements may be illustrated with the human arm. Let any one hold steady the portion from shoulder to elbow, which is to represent the fixed tube down which the observer looks: he is supposed to be perched at the shoulder. Now crook the elbow (which gives the name *coudé*

to the telescope) to a right angle, and imagine a mirror placed at the elbow, so that light coming from the fingers down the forearm is reflected by the mirror at the elbow to the observer at the shoulder. The lens of the telescope is placed at the fingers, where there is also a second mirror, which gives command over different stars. A telescope, in order to reach any part of the sky at a given moment, must have at least two motions. In the equatorial *coudé* one of these is given by this mirror near the lens (at "the fingers"), the other is obtained by rotating the whole instrument round the elbow-to-shoulder portion as axis. The rotation is effected by clock-work in the same way as for any other equatorial, so that the complete instrument now stands as follows:—A mirror at the fingers, set at 45° with the forearm (and capable of rotation round it as axis), reflecting stars of different *declinations* through the object-glass down the forearm; another mirror at the elbow, which reflects the light up to focus at the shoulder, and the whole moveable by clock-work in *right ascension*, *i.e.*, round the axis of the elbow-to-shoulder portion.

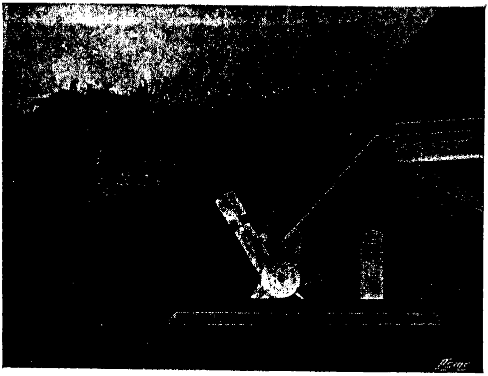
There are other devices of a similar kind in which the same principle is adopted—viz., that the observer should remain in a constant and comfortable position, while the movements of the instrument are still under his control. A fine telescope of this class has recently been set up at the Cambridge Observatory; but it differs from the *coudé* of M. Loewy in having only one mirror at the elbow, the motion in declination being supplied by varying the angle at which the forearm is crooked, and at the same time varying the position of the elbow-mirror at half the rate; a condition which involves a little mechanical ingenuity, but has been found perfectly feasible. Two illustrations of this telescope are given. In one the portion of the telescope out in the open air is shown, the elbow being considerably crooked. To cover up this part of the instrument when not in use, there is a shed running on rails, which has been pushed aside in order to take the photograph. The upper part of the instrument disappears into the small tower for the observer, the interior of which is shown in the second illustration. Instead of lying on his back, or twisting his neck according to the vagaries of the object to be observed, he can

**The Sheepshanks Telescope at Cambridge**

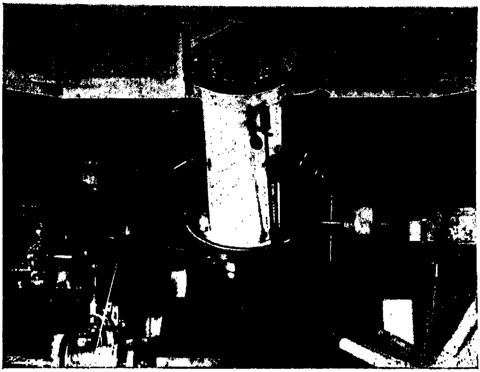
sit permanently in the position occupied at a writing-table; and there is another point of material comfort—the room can be comfortably warmed without affecting the definition of the star images too much; whereas in the ordinary dome the temperature must be kept as nearly as possible the same as that of the outside air, in order to avoid air currents, which spoil the definition of the images; and in the winter this makes observing cold work. It is not easy to represent this difference of temperature in an illustration; but the general gain in material comfort is symbolized by the introduction into the picture of a table on which apparatus is arranged for afternoon tea.

**The Telescope for Amateurs**

To divide a telescope into two portions, one of which can be fixed, is not a new idea, having been adopted in the "broken-transit" and other instruments. Professor Pickering, who uses the principle in his "meridian photometer," remarks with surprise how little use is made of it by amateurs; for it is specially important to those who regard astronomy as an amusement rather than a profession that they should not be gradually weaned from it by discomforts, as is so often the case. Probably



Outside View.



Inside the Observing Tower.

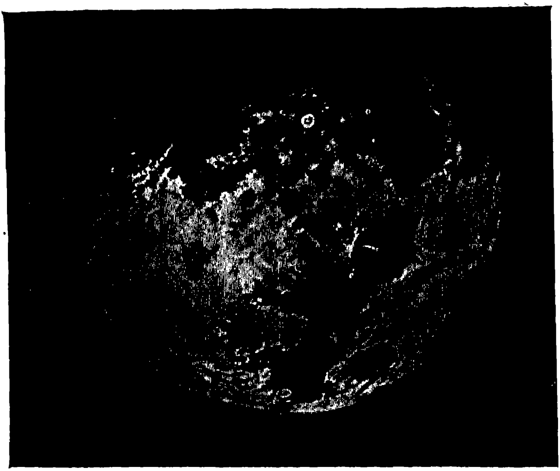
THE SHEEPSHANKS TELESCOPE AT CAMBRIDGE.

they buy their instruments when their astronomical knowledge and acquaintance is slight; they buy a telescope of the ordinary pattern, and set it up some distance from the house in a small and uncomfortable building. Their first enthusiasm carries them out to look at the wonders of the sky, though the night may be cold, and the position for observing cramped; but there comes a time when the key of the observatory is not often required. With some device for bringing the eye end of the telescope into the comfortable study, the fascination of the work would be much more lasting. Professor Pickering, with his meridian photometer, does four hours' observing with the greatest comfort on every fine night. M. Loewy had given up observing at all before he invented the equatorial *coudé*, owing to its discomforts and inclemencies; he had left it to younger men, and occupied himself with indoor astronomy. But when his instrument was built, he went back to it with delight. The matter is worth the attention of amateurs. There is no reason why instruments of this kind should be more costly than others: the only necessary extra is a plane mirror or perhaps two; and it should be easy to devise a form of covering for

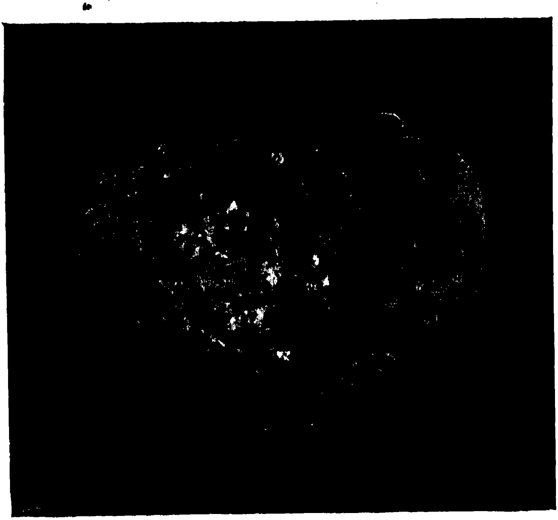
the part of the instrument which is in the open, which will be cheaper than the ordinary "observatory."

**Photography** The greatest changes in all methods of work have followed on the introduction of photography. The patient delineation of objects by hand has been largely (though not entirely) superseded; discoveries are made automatically instead of by toilsome searching; objects too faint to be seen are rendered visible and measurable; sudden phenomena are seized and retained for detailed examination: in numerous ways the work of the astronomer has undergone the most fundamental modifications. The new instruments introduced into the observatory, of which some account was given in the first section, are of themselves sufficient to change its aspect; but the changes in the actual processes of work are even greater.

**Pictures of the Moon** The most obvious use of photography is to give us at once a picture which formerly had to be elaborately and patiently drawn by hand. The observer need no longer sit for hours, perhaps months or years, at the telescope, peering through it for a moment, transferring his impressions to paper; again



RUSSELL'S DRAWING OF THE MOON, 1795.



THE MOON AS PHOTOGRAPHED AT PARIS, 1895.

oking for a little more information, and so ternating between telescope and sketch-book. e has only to put in the plate, watch that ıe telescope is properly guided, and at the end the allotted time he can develop a picture ır more accurate than he could ever draw. here is at the Radcliffe Observatory, Oxford, beautiful crayon drawing of the Moon bear- ıg the date 1795. It is five feet in diameter, nd was made by John Russell, R.A., who de- oted to the work all suitable nights which he ould possibly spare for nearly eighteen years. is telescope was a 6-inch reflector lent him y Herschel, and he undertook the work at the ıggestion of the President of the Royal ociety. There is no doubt that it represents ıe utmost that skill and labour could produce century ago. In some ways the value of his drawing will never be superseded by hotographs; but a photograph can now be btained in a second or two which is *for many urposes* much better than the result of these ighteen years of an artist's work. In the lustration a copy of Mr. Russell's drawing is hown alongside one of the beautiful pictures f the Moon taken at the Paris Observatory vith the equatorial *coudé*; a good deal of

detail is, of course, lost in reproduction, but a fair comparison of the two can be made. The photograph is not always superior to the drawing; there are small details which the eye can catch that are lost in the photograph. The very faithfulness of the photograph is in some respects a disadvantage: it catches the whole surface of the Moon just as it is at a particular moment, but only at that moment. Now the artist cannot do this: the sunlight and shadow on the Moon's surface are continually changing, and while he is drawing one part another will have altered. Hence he cannot rival the accuracy of the photograph. On the other hand, he can, by watching the changes under different illuminations, get an idea of the real shape of objects, and perhaps convey it in his drawing, which thus becomes to some extent the equivalent of several photographs.

**Pictures of the Sun**

This advantage of the artist only exists, however, when the changes are regular and due to known causes, such as the rising and setting of the Sun on the lunar mountains. But there may be irregular changes which he cannot allow for in this way, even on the Moon. And there are other bodies subject to un-

doubted changes in form or structure. The surface of the Sun, for instance, undergoes real variations with sensible, and some of them with startling rapidity. Most people have heard of Sun-spots, though they may not know of their caprices. We find from observation that the average number of spots on the Sun is subject to periodic fluctuations in about eleven years, but we are almost as far off as ever from knowing what the spots *are*, or what causes them. A spot appears on the disc, grows, lives a time, fades away and dies, like something organic; or perhaps we might say as a storm comes and passes. During its life-time the spot is carried across the Sun's disc by the rotation of the Sun; it may be carried across several times, disappearing for the interval when it is on the face turned away from us. Thus the Sun's surface is constantly changing, and a photograph, which can be taken in the hundredth part of a second, or even much less, has obvious advantages over a drawing requiring a considerable time to make.

**Comets and Nebulæ**

In the case of comets and nebulæ also the draughtsman has been almost entirely super-

seded by the photographer; for in these objects there are delicate details which it is beyond the skill of the draughtsman to portray. Especially is this the case with regard to the relative brightnesses of different parts: photographs show these to have been strangely misrepresented by the draughtsman. There is a very good reason for this fault: the draughtsman only sees a very little of the object at one time. The eyepiece of a large telescope only admits light from a very small portion of the "field"; and when another portion is to be examined, the eyepiece must be moved or the telescope bodily moved. Professor Barnard devised a few years ago a striking lecture experiment. He threw on the screen what he told us was the Great Nebula in Orion; but all that we could see was a small patch, the rest of the screen being dark. He explained that the small patch was all that could be seen of this nebula *at one time* with the great 36-inch of the Lick Observatory; but he showed us that the nebula was really there by moving about the small illuminated patch (which was formed by an aperture in an opaque cover) to different portions of the picture, much as the eyepiece could similarly be moved about. We were in

this way able to follow the well-known form of the nebula. But what a difference when he removed the opaque cover and showed us the photograph in all its grandeur! It was a veritable revelation! The photographic plate, which receives the impression of the whole picture simultaneously, is at an immense advantage compared with the eye, which must wander from point to point, forgetting, or modifying by recollection, what it has already seen.

**Planets**

But having shown how severely the draughtsman is handicapped in his competition with the photographer, we must now notice one case in which he still holds his own; which is in pictures of the major planets. The reason is, that owing to the restlessness of our atmosphere the very fine detail on these planets is only visible by glimpses. Now the photographic plate can take no advantage of glimpses; it goes on steadily recording during the whole time of the exposure; good views and bad views are superposed, and the result is a confused or blurred image, in which the fine detail is lost. It has, indeed, been proposed that an observer should attempt to take

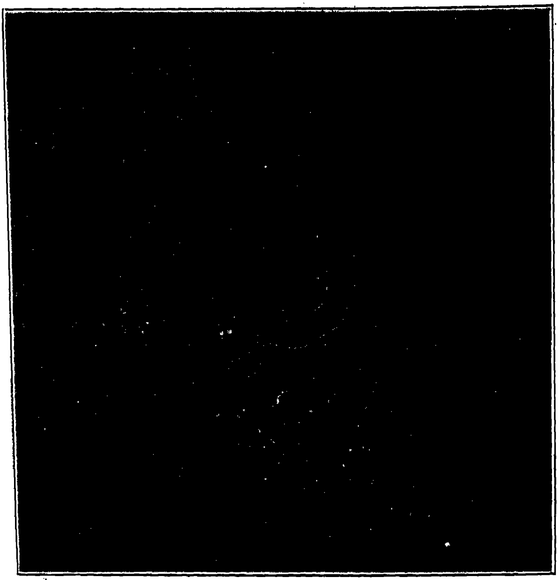
advantage of glimpses even photographically; that he should watch the surface through another telescope, and only open the shutter of the camera at the good moments. But this would require marvellous quickness of hand and eye, and even then might not attain its object; for the air tremors which blur an image are very local, and there might, for instance, be disturbance of the image in the photographic telescope just when the rays coming through the other were steadiest. I do not know of the method having been tried with any success.

**[Di]scovery of Minor Planets**

Besides giving us faithful pictures of known objects, photography affords a simple method of discovering previously unknown ones; and this is well illustrated in the case of the minor planets or asteroids. So many of these are now known that it is amazing to think that search was made for such objects for years before even one was discovered. The first discovery was made on the first day of this century 1801, Jan. 1, and three others were found within a few years; but for three decades after this no new planets were found, in spite of considerable labour spent in looking for them.

Knowing as we do now, that there are hundreds, perhaps thousands of these bodies, this ill-success seems strange. But it is by no means an easy matter to recognise these objects without the aid of photography. They are mere points of light, indistinguishable from the surrounding stars except by their slow motion among the stars; and to detect this, methodical measurements and very careful observation indeed are required. There are few people who have the faculty of remembering a configuration of stars so accurately for a length of time that they can recognise the movement of one individual; and yet this *was* the only way of discovering minor planets. With photography to help the case is now very different. If a prolonged exposure is made on a region of sky, and the telescope is properly guided, so that the fixed stars show round images, an object which is moving among them will leave a "trail," as shown in the illustration. (A ring has been drawn round the trail to draw attention to it: this ring must not be mistaken for part of the photograph.) In this way minor planets betray themselves automatically. Dr. Max Wolf, of Heidelberg, has already discovered over three score in this way; and the illustra-

tion is a copy of his photograph which revealed the planet Svea. There is a delightful simplicity about this method of exposing a plate and looking to see what trails are caught: Sir Robert Ball has called the arrangement a



DISCOVERY OF THE PLANET SVEA (MAX WOLF).

"star-trap." There is one attendant danger:—that an accidental mark or stain on the film may be mistaken for a planetary trail; but this is easily guarded against by exposing another plate and comparing the two.

**Discovery of Comets**

It may be added that comets have been caught in the "star-trap"—by exposing a plate at a venture and examining the objects photographed—but not often. Perhaps more will be done in this way in the future: there is no doubt of the feasibility of the method.

**Photographs of Meteors**

Sometimes, too, meteors have been caught by the watchful plate as they flash by; and here the superiority of the plate over the eye is obvious. The record on the plate is permanent: that on the retina begins to fade at once, and unless immediately converted into a note or a diagram on paper is soon irretrievably lost. Even then the transference to paper carries with it serious imperfections. The photograph is superior in every point save one, viz., if the meteor is not a bright one, or flies across too quickly, its light may not leave any impression on the plate. Films are still increasing in sensitiveness, and it is not known with accuracy what we can do with our present ones. Much was hoped from photographs of the Leonids last November; but as you are aware the result was a failure, not so much from want of sensitiveness in the films as from want of meteors to experiment upon; and this

department of photographic astronomy is still quite in its infancy.

**Eclipses**

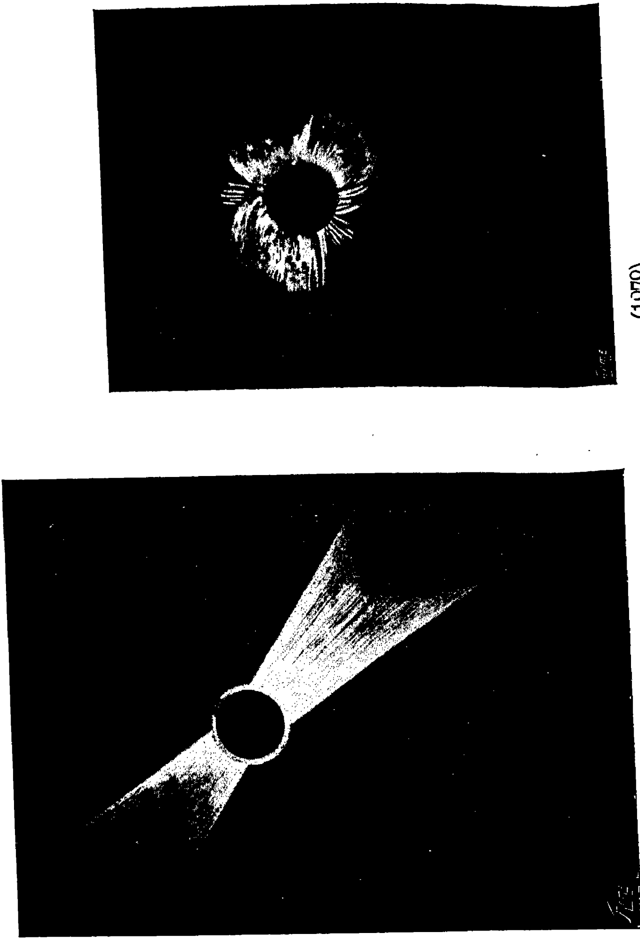
But there are other important phenomena occupying short intervals of time, in recording which photography has already proved itself of immense value—for instance, eclipses. There are four kinds of eclipses: total and partial eclipses of the Sun, and total and partial eclipses of the Moon. But when an astronomer speaks of eclipse work or an eclipse expedition, he generally refers to a total eclipse of the Sun only, which far transcends all the others in importance, being an opportunity of a special kind. When the ordinary Sun is completely hidden by the Moon, there flashes into view the glorious appendage to the Sun called the Corona. It does not appear until the last trace of ordinary sunlight is gone, and it vanishes with the first returning ray. The interval between is only a minute or two, and the occasion only occurs about once in two years in strictly limited localities, which may involve journeys of thousands of miles to reach them. Yet these brief and rare intervals are the only opportunity we have for studying what is really

far the most interesting part of the Sun. In the year or two between eclipse and eclipse the corona is for us entirely hidden from view by the glare of the sunlight on our atmosphere; and we can only conjecture its history from the brief glimpses afforded us on the occasions of a total solar eclipse. Small wonder then that we desire to make the very most of our opportunities, and to this end photography has been invaluable. Eye-observations of the corona and its spectrum were hurried and affected by intense excitement; the photographic plate records its impressions with lightning rapidity, but without the least flurry. Drawings of the corona were even more unsatisfactory than those of nebulæ; for they could not even be made patiently. Photographs of the corona are better than those of nebulæ, for the object is brighter, and shorter exposures can be given.

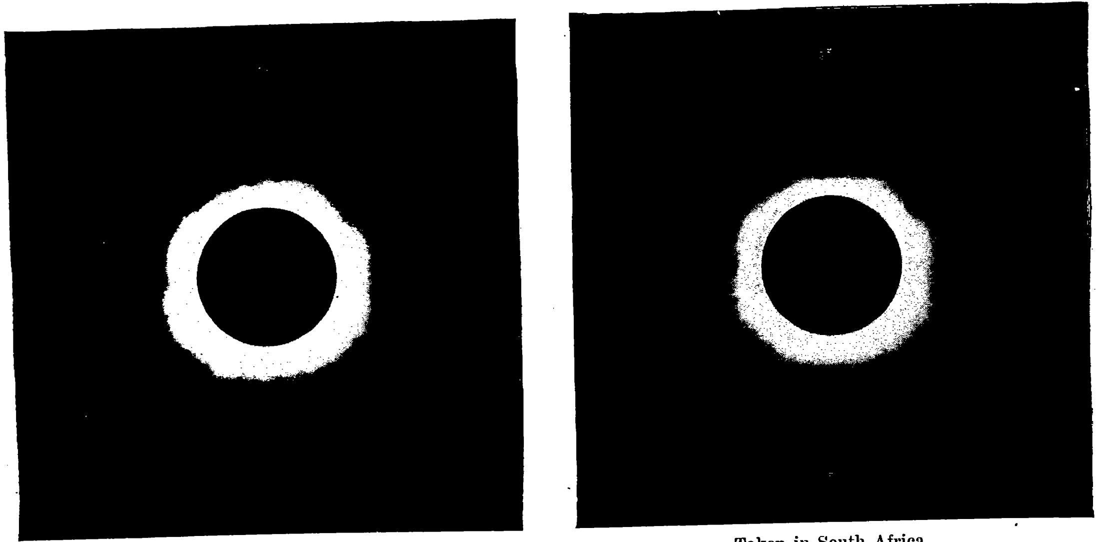
Look, for instance, at the illustration showing two of a series of drawings of the same corona (that of 1878), which are *bona fide* attempts to represent the same phenomenon. They are not exceptional: in the Washington

Observations for 1876 a whole series of similar drawings was published, resembling each other no more than this selected pair. Now look at the two photographs of the 1893 eclipse taken at places some 2,000 miles apart, and you will see the closeness of the similarity. Indeed, it is so close as to be in a sense disappointing. We had hoped that in the interval of an hour between totality at these two widely separated stations, some minute change of structure might have declared itself. But on the most careful examination no trace of such change has been found. Drawings of the corona have recently shewn a considerable improvement, but this is in part due to the education afforded by photographs.

Eclipse work does not consist merely in taking pictures of the corona; the spectrum of the corona is also scrutinized, and its light is examined for polarization. But in this and other eclipse work photography has become almost imperative. The eclipse observer no longer trusts his eyes and excited brain during the few precious minutes of totality: his skill is exercised in preparing



TWO DRAWINGS OF *THE SAME* CORONA (1878).



Taken in Brazil. Taken in South Africa.

TWO PHOTOGRAPHS OF *THE SAME* CORONA (1893).

beforehand, with the utmost care, a programme of work which shall give him the best photographs. This programme, which may require the help of several persons to carry out, is rehearsed by them over and over again, until the operations can be performed like clockwork; and then, when the critical time comes, the observer and his assistants become as like machines as possible, and are relieved from the strain of making observations.

**Relief of the Observer**

There is a story of Sir George Airy which brings home to us the difference between the old order of things and the new. A distinguished American astronomer, who had come to Europe to observe a total eclipse, paid a visit to Greenwich on his way. In taking leave of him Airy said, "Well! good-bye; and I think the best wish I can wish you is—cloudy weather! I have been on several eclipse expeditions, and on the whole the most satisfactory ones have been those when we saw nothing because of clouds." In this rather cynical remark there was considerable wisdom. Eye-observations made so quickly, and under the strain of anxiety

and excitement, were liable to errors and uncertainties which sometimes rather hindered than helped the progress of our knowledge. The observer felt bound to do his best, but if clouds prevented any observations at all, he had at least the satisfaction of knowing that he had made no mistakes. With photography to help him, however, and proper time for preparation, he need have less fear. He can make the operations almost automatic: indeed, one enterprising eclipse observer, Professor D. P. Todd, has devoted himself to proving that they can be made absolutely automatic. He arranges an immense amount of apparatus so that it can be played like an organ, by pneumatic arrangements controlled by clockwork; and after putting all this in order he can, as he says, go quite away at the time of the eclipse, knowing that his machinery will take all the photographs he wants in the proper way. Thus equipped he did anything but hope for cloudy weather; but, by the irony of fate, cloudy weather seemed to dog his footsteps. Three times he set up his apparatus at three different eclipses and was disappointed by the weather; he

merely had the satisfaction of knowing that all had gone perfectly, and *if* there had not been clouds he would have got excellent photographs. At last, however, after having carried his apparatus some 50,000 miles altogether, the weather ceased to persecute him, and in May last he had the satisfaction of seeing his astronomical organ play a real tune for the first time, in Tripoli. No one else has yet adopted his plan so far as I know; but we all of us make ourselves as nearly like machines as we can during the few minutes of totality. I am tempted to mention a trivial circumstance in illustration. For the total eclipse of August, 1896, we had pitched our astronomical camp in the hotel yard of a Japanese fishing village. The officers and men of H.M.S. *Pique* were told off to assist us, which they did in the ablest manner. Some of them were to hand the plate-holders to the operators and others to receive them, so that no precious fraction of a second should be lost: others were to screen the lenses until the word was given to make the exposure: and two were to count out in a loud voice the seconds of total eclipse, "One, two, three," etc.

All these operations were practised several times on the days before the eclipse, so that everything should go without a hitch: and a crowd of merry little Japanese round our enclosure watched and listened with the greatest interest. During one of our subsequent walks we were startled to hear the voices of the time-keepers imitated with wonderful distinctness "One, two, three," etc.; and on looking round saw a party of Japanese children saluting us in these familiar terms. We had rehearsed so often that they had learnt the performance!

**Star-charting**

To the above-mentioned uses of photography (for quickly getting pictures or portraits of complex structures, for discovery of new planets and comets, and for recording transient phenomena), we have now to add its uses in the domain of exact astronomy, where until recently only measurements made by the eye of a skilled observer were trusted; and especially in recording the relative places of the stars. It may seem as though this were not a new use; that to take a picture of a group of stars is not essentially different from taking a picture of a nebula or the

Moon; and this is quite true. The difference between the cases lies in the degree of accuracy required. It was long thought (and the notion was only dislodged with difficulty, indeed it is largely prevalent still), that while photography was capable of making a picture in which the features were exhibited roughly in their true relations, the resemblance to the original was not accurate enough for astronomical purposes. There are curved mirrors in which the curious may see their own faces grossly distorted, and yet recognise the likeness: the face is too long for its width perhaps, but eyes, nose, and mouth still occur in their proper sequence. So it was thought that there might be distortion in the pictures obtained photographically—not gross distortion, but still enough to render measures made on a photograph rather than on the sky only approximate. It seems difficult, now that we know how accurate a photograph really is, to understand this old mistrust of it; but of course it was only prudent to make sure of the new weapon, before preferring it to the old.

**Former Distrust of Photography**

Although confidence in the accuracy of photographs is now firmly established, there is one material symbol of the old lack of faith which will probably remain with us, because it is eminently useful, though in a different way from that which was expected. The *réseau* (a network of lines at right angles which is impressed on star photographs, of which mention was made on pp. 75, 110), was originally proposed as a safeguard against the distortion of the film—one of the faults of which photographs were suspected. Experience shows that it is unnecessary for this purpose, for such distortion is insensible; but it has been found so useful in measuring the plate that it will probably become, if it has not already become, a permanent feature in photographs intended for accurate measurement. It dates from the Astrographic Conference of 1887, of which something will now be said.

**The Astrographic Conference of 1887**

The Astrographic Conference of 1887 met in Paris to consider the project of making a complete map of the whole sky by international co-operation. The credit for initiative falls chiefly to two men—Sir David Gill,

of the Cape Observatory, and the late Admiral Mouchez, Director of the Paris Observatory. At both their observatories work had been done which showed the great promise of the new method, and they felt that the time had come for decisive action. The Conference was a great success. A standard pattern of instrument was chosen, and eighteen observatories undertook to get instruments of this pattern and take part in the work.

There were about 45,000 plates to be taken, and accordingly the share of each observatory is to take about 2,500 plates, half of which are to have a long exposure of nearly an hour, showing on the average 1,000 stars per plate; the other half a short exposure, which, therefore, only gives the brighter stars, to the number of about 300 per plate on the average. Even these "short exposure" plates will exhibit an enormously greater number of stars than have ever been recorded before in any way.

**Scale of Chart**

A complete map of the heavens is not, of course, an entirely new thing; this International Chart will only be new in the scale on which it is made and the detail shown.

At the beginning of an atlas of geography we find a map of the whole world on a small scale, and of the towns in England perhaps only London is marked on this map. After this we find a map of Europe, in which the chief English towns are noted; but later comes a map of England itself, with many more towns and even villages; and if we care to get the large scale Ordnance map we get vastly more information still. The International Chart is to bear the same relation to previous charts of the heavens as the large scale Ordnance map does to a small one from a school-atlas; it is to be immensely bigger and very much more accurate. The most extensive map of the stars at present in existence, due to Argelander and Schönfeld (and this is only made for about half the sky— the northern hemisphere and a little of the southern), would contain about eighty sheets of paper (size 29 in. × 21 in.), and weigh about a stone if completed for the whole sky. The International Chart, if completed, will contain *ten thousand* sheets, forming a pile of paper twenty feet high and weighing nearly *a ton;* and it is not too much to say that the star positions will be indicated with an accuracy

ten times as great as that of Argelander, and there will be at least ten times as many. Such figures will give some idea of the magnitude of the undertaking. It is far from completion as yet; but its course has been prosperous enough to give every hope of ultimate success, and all those who helped its inception are to be congratulated on the realization of a noble work, which is bearing quite unexpected fruits in various directions.

The names of Admiral Mouchez and Sir David Gill have already been mentioned, but France generally deserves a great deal of credit. It was the brothers Henry, working at the Paris Observatory, who devised the form of instrument adopted as a general pattern, and made it with their own hands; and it may be added that it was in the course of completing a previous French enterprise (Chacornac's ecliptic charts) that they thought of using photography at all. The French, with their well-known hospitality, have entertained in Paris not only the original Conference of 1887, but several subsequent meetings of the Executive Committee, which is almost the same thing; and they contribute a larger

number of participating observatories to the eighteen than any other country, unless we include Colonial observatories as English.

**Measures of Star Places on Photographs**

A most valuable outcome of the enterprise has been the demonstration of the rapidity and ease with which stellar positions can be determined by measures made on photographic plates. As an instance in point, the Cambridge catalogue of stars, published a few years ago, gives the positions of 14,000 stars in a certain narrow belt of the heavens. This represents twenty years' work of two people with the transit-circle. It falls to our lot at Oxford to explore the same belt of the heavens by photography. We shall, perhaps, have six people at work at Oxford, but to give a simpler comparison I will divide their work by three. With a staff equal to Cambridge we shall, in five or six years, obtain photographically the places of two or three times as many stars; in other words, the work is done five or six times as quickly, and the results are even more accurate.

Much of this gain in rapidity is due to the fact that the study of star photographs has taught us the inconvenience, in some connec-

tions, of a method of work which has been hitherto universal in astronomy. Maps of the stars have hitherto been made by the use of instruments, especially the transit-circle, which utilize in some form or other the rotation of the Earth; and, in consequence, they are covered with sets of lines like those on maps of the Earth, one set converging to the poles and another set curved in some way or another. The use of such reference lines for star positions complicates the calculations, and it is much simpler to use straight lines at right angles, such as those given by the *réseau* (see p. 75) on a flat plate. We cannot use such lines in geography because the Earth really is spherical and has two very real poles; and accordingly our reference lines must be curved over its surface, and the poles are important points. But the poles do not belong to the stars, they only seem to belong to them because we use terrestrial instruments, and we can make an equally good, or better, map of the stars with poles in quite different quarters of the heavens from those to which our terrestrial poles point. And, further, though it has been customary to consider the stars displayed upon a sphere, this is only a

convention which can be discarded in favour of any other more suitable. In dealing with photographs it is far more suitable to regard the stars as displayed or projected on a flat surface—indeed the very operation of photographing them produces such a "projection" (in mathematical language the stars are projected through or from a point, the centre of the lens, on to a plane, the photographic plate); and the introduction of the *réseau*, originally for quite a different purpose, has shown clearly how much is gained by shaking off the trammels of the old system, and working with plane rectangular co-ordinates, instead of with the right ascension and declination to which astronomers had become so thoroughly accustomed.

**Measures of Lunar Photographs**

With some modifications this change of method is applicable also to measures on the Sun and Moon, and may make a considerable difference in our knowledge of the Moon's surface. Accurate "selenography," as it is called, is in a most backward state, owing to the laborious nature of the necessary calculations, when any measures have been made. Even with photography to help not much

has been done until quite recently. The late Professor Pritchard, of Oxford, made a large number of measures of lunar photographs, but never published the results; the calculations were so laborious that I do not think they were ever completed.

With the methods of calculation above referred to, which have since been developed, Mr. S. A. Saunder has, within the last year, obtained very satisfactory results from measures on lunar photographs, and he is now entering on a campaign which may result in putting our knowledge of selenography on a more accurate footing.

**Co-operation**

He has another great advantage over earlier measurers of lunar photographs. Professor Pritchard measured the comparatively small pictures, of 1½ inches in diameter, taken with the De la Rue reflector at the Oxford University Observatory in the years 1876–1879. These are rather small judged by modern standards; and pictures taken with a reflector have apparently some defect of distortion which does not appear with a refractor. (Such, at any rate, is the experience at Greenwich, at Oxford, and elsewhere.) Now Mr.

Saunder has been able to measure some of the wonderful 6-inch pictures of the Moon taken with the large equatorial *coudé* at the Paris Observatory, which were kindly lent by the director, M. Loewy, for the purpose. It is not the least of the advantages of photography that it affords new opportunities for collaboration between observatories. The staff of an observatory may have fine instruments and good opportunities generally for taking the plates, but not time to measure them in detail. This work can, however, be undertaken by some one else less fortunately situated as regards cameras, but with consequently the more leisure for measurement and examination of plates. A conspicuous example of the success of this new bond between astronomers at a distance is afforded by the *Cape Photographic Durchmusterung*, which is a huge catalogue of stars in the southern hemisphere, constructed by Sir David Gill at the Cape of Good Hope, who took the photographs, and Dr. Kapteyn, of Groningen, in Holland, who measured them. At the University Observatory, Oxford, we have recently carried out a considerable investigation by measuring plates taken at Harvard University

Observatory, U.S., and so on. Such instances will doubtless multiply rapidly in the future as the new conditions are better realized.

**Frequent Charting of the Sky at Harvard**

The mention of the Harvard University Observatory reminds us that the charting of the whole heavens is being conducted at that observatory, in a manner differing widely from that initiated by the International Conference of 1887. We have already compared this latter scheme to an Ordnance Survey extended to the whole earth. Minute details are to be shown (*i.e.*, not only the brighter stars, but the very faintest), and the scale is to be considerable. The total labour and cost will also be considerable. There is no doubt of the value of such a survey, but it is liable to one defect: a large scheme like this, when carried through, leaves a feeling that we may now rest on our oars for a while; astronomers may feel that when the chart is made, their work is done, instead of only beginning; their real business is to note *changes* in the heavens, for which the chart is only the starting point.

Now the energetic director of the Harvard Observatory has begun to accumulate material

for noting changes. He charts the whole sky once a month! Not, of course, on the large scale chosen for the International Chart, but on a scale quite sufficiently large to give valuable information. More than this, with a smaller instrument, and on a smaller scale still, he charts the brighter stars every fine night! So that if a star brighter than the sixth magnitude appeared in any quarter of the heavens, he would have a record of it on the first fine night. Already he has had the satisfaction of such an experience, for when a new star was noticed in the constellation Auriga on February 2, 1892, Professor Pickering found the star on thirteen of his plates between December 10 and January 20. We may remark that he did not accelerate the actual *discovery* of the star, which was made independently; but when made, he was able to say how long the star had been shining before Dr. T. D. Anderson, of Edinburgh, actually noticed it. Had it been possible to completely examine each plate soon after it was taken, the actual discovery might of course have been made from the plates themselves. But this is an immense labour, exactly equivalent, of course, to examining the whole

sky each fine night, and beyond the limited resources of the staff of any observatory.

**Discovery of Eros**

The point is an important one, and worth full consideration. Let us first take another illustration of a striking kind. Reference is made several times in these pages to the discovery of the minor planet Eros, which has come very close to the Earth this winter, 1900–1, and thereby gives us an exceptional opportunity for determining the Sun's distance. The planet was discovered in the autumn of 1898, and soon afterwards it became clear that it had made a *very* near approach to the Earth in 1894. Professor Pickering turned over his vast store of photographs, to see whether he had caught the planet on any of the plates unknowingly; and after a little trouble, which need not be noticed for our present purpose, he found it on several plates taken in 1894, and on others taken in 1896. Copies of some of these plates are shown in the Paris Exposition this year, and on one of them especially the planet is shown by a conspicuous trail. If by a happy chance this trail had attracted sufficient attention, Professor Pickering would have discovered

Eros in 1894, in time to make some use of the great opportunity; and one cannot help dwelling a little regretfully on this "if." It may be asked how a trail of this kind came to be overlooked. The answer is that the number of plates taken at Harvard is so great that systematic examination of them is impossible. An enormous amount of examination is carried out. Professor Pickering and his assistants have done more than all the rest of the astronomical world put together to indicate, and to carry out, schemes for the comprehensive and rapid survey of numbers of photographs; but even then more plates are obtained than can be examined. If he had made it a rule not to take plates without thoroughly examining them, the only result would have been, not that Eros would possibly have been found in 1894, but that the plates on which it was found subsequent to the discovery would never have been taken—not a possible gain, but a positive loss. Professor Pickering has the courage to take thousands of plates, on the chance that they may turn out useful, though he can only, for the present, put them on the shelves. The finding of Eros in 1894 is a conspicuous instance

**The Librarian Policy**

of the success of this policy, which is, after all, very similar to that of the librarian. Some men never buy a book unless they can read it at once ; others form a library of thousands of volumes, most of which they know they can never open ; and both policies have their merits. Professor Pickering is, *par excellence*, the astronomical librarian, and that the first book telling of the existence of Eros passed on to the shelves without being read, was all in the way of business. He and his assistants glance through all the books as they come in, but they naturally cannot read more than a few; and very thorough reading would have been necessary to find Eros in this way. For it must be remembered that the existence of a trail on a plate, though it means a planet, does not always mean a *new* planet. There are more than 400 already known, and it is necessary first to make sure that it is none of these—in itself a laborious piece of work. Then, again, no one suspected that any of these bodies was going to turn out particularly important—interest in them was beginning to flag. Would it be pushing an analogy too far to compare minor planets with minor poets? We may liken Eros to the one poet

of a century who suddenly emerges from the crowd. Professor Pickering is the librarian, who then finds he has on his shelves several early productions of this new "star," but this does not mean that he ought to have thereby "discovered" him.

This point is dwelt on at some length, because, as above remarked, it is a most important one, as those familiar with the history of astronomy will recognise. One of the commonest faults in the past has been the accumulation of observations which were never reduced and published. The temptation was to use the fine nights at the telescope and trust to luck for the reduction of the results: too often it turned out that if only a fraction of the observations had been made, and the time and energy saved from the remainder devoted to the discussion and publication of these few, the world would have been the wiser. One of the great reforms introduced by Airy was the prompt publication of the observations made; and though his example has had a most salutary effect, lapses are by no means unknown in modern times. Is the taking of photographs and placing them

on the shelves a repetition of the old error? There may be some who would reply in the affirmative, but I fancy the general answer even now would be a distinct negative, for the reasons above given; and I feel sure that in a very few years there will be no doubt on the subject at all. If so, we could not have a more striking instance of contrast between the old methods and the new.

**Rapid Examination of Plates**

At the same time, it is clear that methods of examining photographs rapidly must be devised, if possible. We have at our disposal material of which we can only take a fraction of the full advantage; and it behoves us to look about for more efficient methods of dealing with it. The pioneers in this respect are to be found also at Harvard, as has been already remarked; and one of their ingenious devices is worthy of notice. If two photographs be taken at different times and superposed, so that the two images of the same star are close together, each pair of images may be made to form a "double star," with the images similarly separated; and so long as the two plates are merely examined by eye, which has an adaptable focus, both

images of the pair can be fairly focussed. But we really see one set of images through the glass of the upper plate, and if we tried to make accurate measures with a microscope, this would be found a serious difficulty: the focus would be sensibly different for the members of each pair. If we put the plates film to film, we should not, of course, be able to make more than two images coincide, and to make a positive from one plate and put it film to film with the other is not satisfactory. Professor Pickering's device is to put one of the plates into the telescope with *film away from the object-glass*, so that the rays of light pass through the glass plate to the film. The difference of focussing is very slight, and may be neglected for the purpose of finding conspicuous changes; and the resulting picture has the same sort of inversion with regard to an ordinary plate as a positive has to a negative, viz., right is changed for left, while up and down remain the same. Thus it can be placed film to film with a plate taken in the ordinary way on another night, of the same region, and all the pairs of double stars formed by the two sets of images will be very approximately at the same distance from

**The Film to Film Device**

the examining microscope, and hence in focus together. Any case of large proper motion or parallax in a star would give an unusual distance or position-angle to the corresponding "double star"; and any case of variability in brightness would make the two components unequal. That a rapid review of a region can thus be successfully conducted has been proved by the results: for instance, the well known star 61 Cygni, which has a large proper motion, was recognised at once, although the reviewer had no idea even of the region of sky under examination. Again, variable stars have been actually discovered during a review of this kind: one of them being of special interest as a short period variable.

**Paucity of Peculiar Stars**

That there are not many examples to be quoted is in some ways disappointing, but has a definite significance: it must mean that the number of exceptional objects in the sky is not great. This particular kind of dredge-net has been dragged over portions of the sky without catching much. The net is presumably a good one, since it has caught automatically what we knew to be there already,

and even made new captures. If the haul has not been a large one, we may conclude that there is not much to catch; and, after all, there is considerable comfort in this thought. We already have our hands very full, and if it were rendered probable that discoveries might easily be made in large numbers, other work would undoubtedly suffer, unless the astronomical army were suddenly reinforced by a large body of new workers, which does not seem immediately probable. As it is, some method of this kind offers to those who may be willing to undertake a straightforward, though laborious search, a practical certainty of sooner or later making some interesting discovery without any apparatus at all beyond a simple lens or microscope.

There are plenty of photographs already in existence to occupy several lifetimes in such examination. The Harvard Observatory alone has a practically inexhaustible store, and Professor Pickering has already and several times expressed his willingness to furnish material for any earnest worker. As has been already remarked, a man of any length of purse, or with no purse at all, may be an astronomer and a discoverer now-a-days.

**Photography in Meridian Astronomy**

So far we have considered photography chiefly as an aid to *discovery* and *record*: discovery of new minor planets or other objects; discovery of changes—proper motions or variations in brightness. Star-charting is the first half of a discovery in a sense, but may be better called a record; pictures of nebulæ are records, though often also considerable discoveries.

But photography has also lent aid already, and is certain to lend much more in the future, in the astronomy of exact measurement. Sooner or later the photographic plate is bound to replace the observer at the eye end of the transit-circle, though as yet there is little progress in this direction to report. Photographic transit-circles are indeed already in existence, and other forms have been proposed, but most of the meridian work in the world is still done by eye. It must not be too hastily assumed that this is to our discredit. The merits of the visual transit-circle have been established by a century of work: it is not to be lightly abandoned for an instrument as yet scarcely designed. True, it has defects (or rather the man who uses it

has defects), which can be obviated by photographic method; but we have yet learn the defects which must also inevitab come with the new instrument, and it will wise to know, and if possible to cure the before abandoning the existing and prov instrument.

**Longitudes by Photography**

Before describing the general principle which I believe a photographic transit-circ will ultimately be constructed, I will ref to a case in which photography has bee actually used with success for one of tl problems of the old astronomy. It has bee remarked that a traveller or sailor finds tolerably easy to determine his *latitude*, bı a difficult matter to find his *longitude*. F the latter he wants to know the Greenwic time; and if he has not carried it with hi from civilization, in the shape of a good-goin watch or chronometer, his best way of findin it (a poor one at the best), is by observin the place of the Moon among the stars. H may do this in a variety of ways so lon as the main object is attained. The sailor i provided in the *Nautical Almanac* with table of "lunar distances," *i.e.*, distances of the Moo

from certain well-known bright stars. Owing to the Moon's motion these distances are constantly changing, and the Greenwich times, when they have certain specified values, are known. Hence, if the traveller measures one of these distances at a certain moment, he knows the Greenwich time. Let us suppose it is exactly midnight with him, and he finds that the distance between the Moon and Aldebaran is that given by the tables for 10 p.m. at Greenwich: then he knows that he is two hours East of Greenwich. The defect of the method is its inaccuracy, owing to the slow (and, it may be added, to the imperfectly known) motion of the Moon, and to the difficulty of making exact observations with the sextant, which is only provided with a very small telescope. If we could use a larger telescope we should increase the accuracy; and here, as elsewhere, we gain by having an automatic record by photography instead of human observations. Capt. Hills, R.E., has accordingly devised a method of making this observation photographically. Remembering that the problem is to determine the place of the Moon among the stars at a given instant photographically, there are

certain difficulties to be overcome. The Moon is so bright an object that it is almost out of the question to expose a plate to the Moon and surrounding stars and expect to find them both shown. If the exposure were long enough to show the stars the moonlight would in general fog the plate all over. The photograph is therefore taken in two operations: first, a snap-shot is taken of the Moon with a camera which is left firmly fixed; after an interval (say one hour), the Moon's image has passed off the plate, and if the shutter be now again opened, stars will shine through the lens—not those immediately round the Moon, but others at a known distance from them, and these can be photographed without disturbance from moonlight. By choosing the interval properly we can also get bright stars. And by measuring the place of the Moon among these stars which we have artificially brought alongside it, we can infer its real place, and so find the Greenwich time, when there is no telegraph line.

In order to leave a trail on the plate a star must exceed a certain brightness. We can photograph very faint stars when the camera

is made to follow them by means of clockwork; but it is the essential part of the above method that the camera should remain *fixed*.

**Photographic Transit-Circle**

This limitation can be removed by an artifice. Let the camera be firmly fixed, with the exception of the plate, which can be moved in a slide by clockwork so as to follow the motion of the stars. We can then keep the image of a star shining on the same point of the plate for several minutes, and so faint stars will no longer be lost. We have, however, introduced an uncertainty, for the plate will not be in the same position when the Moon is photographed as when the stars are photographed, and we must know the relation between these positions. For this purpose let a spot of light from a fixed source fall on the plate; and let there be a screen which can cut off this beam every second (a good clock can be arranged to do this automatically). Then, as the plate moves along, the spot will form a line of dots, which register the position of the plate at each instant, so that we know its relative positions at any two moments. In this way the advantages of a moving plate and a fixed

plate are combined. This principle is of wide application, though it has not yet been used in practice.

It may be applied to either the transit-circle or the almucantar. Thus: a camera is fixed in a vertical position, lens downwards. The plate can move in a slide, which can be turned into any azimuth, and the rate of motion of the plate is controlled by a lever. Under the lens, reflecting the light from stars into the camera, is a plane mirror. If this mirror is mounted on pivots, and capable of rotation round a horizontal axis placed east and west, we have a photographic transit-circle. If it is floated at an angle with the vertical we have a photographic almucantar.

**The Photo-Chronograph**

Photography has, however, been applied to the transit-circle in other ways, usually by allowing stars to trail across the plate and interrupting the trail at regular intervals. Excellent results have been obtained in this way at the Georgetown Observatory by Father Hagen. The method is limited to the brighter stars, but positions of the fainter stars can be obtained by other methods. If the difficulties

of photographing stars in the day-time could be satisfactorily got over, I should not hesitate to prophesy that some form of photographic instrument would entirely supersede the visual transit-circle in the near future.

**Variations of Personal Equation**

For visual observations, accurate as they are, have one great defect, that no two observers make them in the same way. Every one has an idiosyncrasy called a "personal equation," which causes him to observe a transit always too early, or always too late, as the case may be. If this were the whole of the story it would not matter much, for we can easily compare the personal equations of different observers and make the proper allowance accordingly. This has been done year by year at Greenwich for nearly a century, assuming throughout that the personal equation of each of the observers remained sensibly constant during each year, and was the same for all stars. But we have lately found that this quantity is *not* the same for all stars: it depends, even for the same observer, on the brightness of the particular star he is observing. Generally speaking, if it is a bright star that is crossing the transit wires he signals its

crossing too early, as if the brightness of the star accelerated the message to his brain. As yet no satisfactory reason *why* this should be so has been formulated, but there is no doubt whatever about the fact, to which serious attention was first called by Sir David Gill in 1878. He had compared the relative places of certain stars of different brightness with his heliometer, an instrument which does not introduce personal equation of the kind we are considering; and he found that his observations could not be reconciled with transit-circle observations except by assuming a variation in personal equation according to brightness of the star. Since then several laborious investigations have been undertaken which have demonstrated this variation, and measured its amount with more or less success. But during the last year or two it has been shown how easy it is to measure this quantity by means of photography. For instance, the belt of the heavens which was observed for twenty years with the transit-circle at Cambridge, is now under survey by the photographic method at Oxford. The distance between a bright star and a faint star as found at Cambridge is slightly erroneous, because the bright star was

**Photographic Determination**

signalled relatively too soon; but this error does not exist in the photographic measures; and so a comparison of the two surveys detects it. It is so small that we must compare as many stars as we can to get a good result on the average: but we have plenty of material —many thousands of stars—and when they were examined it was found how clearly the variation can be brought out in this way. Down to magnitude 8·0 the signals given by the eye-method are given earlier and earlier by about 1-50th second for each magnitude brighter. For stars fainter than 8·0 the change is much larger than this. The observer at Cambridge was the same throughout (Mr. A. Graham), and these results show his individual change of personal equation. But a comparison was made with the general average of the observers at Greenwich, and it was found that Mr. Graham's habits agreed minutely with the average of all the Greenwich observers. Hence we infer that observations made at Greenwich are all affected in this way. There are reasons, which I need not here enter into, why this is not so serious a matter as it looks at first sight; but it is sufficiently serious to demand immediate attention. A photographic transit-

circle would remove the cause of error root and branch.

**Star Magnitudes**

The point just referred to reminds us that the stars differ in brightness as well as in position; and it is important to measure these differences in lustre. Most of the stars remain constant in magnitude, and occasional measures to confirm the constancy are all that are necessary. Others, called variable stars, are continually changing; and from the history of their changes we may hope to learn something of the nature of these wonderfully interesting objects. The few references I can make in this rapid review of the last quarter of a century are out of all proportion to the immense amount of work recently done in photometry. (Harvard Observatory is in a sense devoted entirely to photometry.) And at present we are considering what is new in equipment and method rather than gauging the amount of work done. We accordingly select for notice a method conspicuous for its novelty, although the results obtained by it are as yet few. In the history of telescopes up to the last ten years, probably very few observers

**Images out of Focus**

have ever deliberately put their telescopes out

of focus for a useful purpose. The minutest deviation from perfect focus has been uniformly regarded as an instrumental defect, to be avoided if possible. Recently it has occurred to two people independently (Professor Pickering, of Harvard, and Dr. Schwarzschild, of Vienna) to take photographs of stars *considerably out of focus*, for the purpose of measuring their brightnesses: and the method seems promising for the following reason. When a camera is accurately focussed the image of a star changes in several respects as the brightness increases. The brightest stars give large black discs with indefinite borders: those less bright give smaller discs; and the size of the disc is a good indication of the star's brightness. But for the faintest stars the disc does not diminish in size, it only becomes less black: and thus we must observe not only the size of disc but the blackness of it to get complete knowledge of the star's brightness. Now, if we can reduce these two things to one only, it is a distinct gain: and this object is attained by taking the stars out of focus. The size of the disc does not then vary appreciably, only the density; and we have thus only one element to observe instead of two.

**Motions in the Line of Sight**

Accurate photometry on some plan is very important for the study of variable stars; but perhaps the greatest advance in our knowledge of these objects has come, not from any photometer at all, but from the spectroscope; and the use of this instrument for studying, not the chemical constitution of bodies, but their *movements*, has been one of the most wonderful developments of modern astronomy. It occurred to Sir William Huggins early in his work with the spectroscope, that if a star were moving towards or from us, the lines in its spectrum, which are like particular notes in a musical scale, should slightly change their positions, much as the pitch of a railway whistle alters when it passes us and so recedes instead of approaching. The general principle was not new, especially in its application to sound, where it is called Doppler's principle: but the application of it to light was quite new. Indeed, in 1868 it was not an experimental fact, but only a theoretical probability that the lines in a spectrum would shift their positions according to the motion of the star, and Sir William Huggins entered upon the difficult task of testing it practically. The observations were extremely delicate: the shifting of

the lines is undoubted but very small. The analogy of sound is rather misleading as to the amount of change, because the velocity of sound is so small compared with that of light —little more than a millionth part of it. To get an alteration of colour equivalent to the alteration of pitch of a railway whistle, the train should move a million times as fast, and even the heavenly bodies do not go at this pace. They may move 100 miles a second, but this is not quite 10,000 times the pace of a train. Hence the effects of even these high velocities on the spectra of the heavenly bodies are very slight: one hundred times slighter than the effects on musical pitch of the motion of an express train.

Still they are measurable, when extreme care is used; and the announcement of their detection caused the greatest interest, and in some cases, where it was not quite understood, even alarm. I remember some fifteen years ago, when I was at Greenwich, having to answer for the Astronomer Royal an anxious inquiry whether any danger was to be apprehended from the rapid approach of these stars to us.

When it was explained that they were not necessarily moving straight towards us, and that, even if they were, some millions of years at least would be occupied in reaching us, the inquirer was so obviously relieved that she called down a fervent blessing upon the Astronomer Royal. She would have doubtless been even more gratified by the recent discovery that some of these high velocities are not continuously in the same direction; for by this same method it has been found that some of these stars are revolving. We presume that they are revolving round some star which we cannot see; but that they are moving alternately backwards and forwards there is no doubt. The analogy of sound again helps us here. If any source of sound be in rapid rotation, so that it alternately approaches the hearer and recedes from him, a distinct alternation of pitch can be noticed. There is a schoolboy's implement called a "bull-roarer," which is easily made from a piece of a cigar-box and a bit of string. If in a flat piece of wood, say three inches long and about an inch wide, but tapering slightly, a hole be bored near the broad end and a string pushed through and knotted on the far side, then by

whirling the wood from the other end of the string a buzzing noise or roar is produced by the rapid twisting of the wood. But the sound heard by another person is not a steady one, except in one particular case: it is usually an alternating sound. The alternations are most marked if the observer stands so that the whirling circle is edgeways to him, for then he gets the full effect of the alternate approach and recession at the top and bottom of the circuit. If, however, he stands with his ear in the axis of the whirling circle, *i.e.*, in the straight line through its centre perpendicular to its plane, so that the source of sound is always at the same distance from his ear, he will hear a steady note: the alternation will disappear. The alternation may be made plain to a large number of people at once, but only a limited number can be placed so as to hear the steady note.

**Confirmation from the Sun**

If further experimental evidence is needed to show that similar phenomena occur with light, it is afforded by bodies which are known to be rotating or revolving. For instance, the Sun is seen to be rotating on his axis by the motions of the spots across his disc. The

spectrum of sunlight on one side of the disc is therefore that of an advancing body, and on the other side of a retreating body; if the two are compared, the lines should show relative displacements accordingly. This experiment was made very early in the history of this new method, and found to bear out theoretical reasoning completely.

**Algol**

A more interesting case is that of the star Algol, where the revolutionary motion was not certainly known but only suspected. Algol is a variable star of a peculiar kind. Its light remains constant for two-and-a-half days, then it begins to fade quickly, and after three-and-a-half hours reaches a minimum value, at which it remains constant for twenty minutes: then it brightens again in three-and-a-half hours to its original value. It was shrewdly suspected that the star was really composed of two bodies, a bright one and a dark one, revolving round each other; the variations in brightness being caused by the dark body passing in front of the other so as to obscure it partially though not entirely. It is easily seen how the observed variation of brightness could be explained on this hypothesis, but we

had no direct evidence of the dual existence or revolution. The first attempt to detect a revolutionary motion in the star with the spectroscope on the principle above sketched was made by Mr. Maunder at the Royal Observatory, Greenwich, and he got varying velocities which were distinctly favourable to the hypothesis, though the accidental errors were large. Later Dr. Vogel, of Potsdam, got better observations by photography, and proved the revolution beyond a doubt.

**Saturn**

Another instance is afforded by the planet Saturn and its ring. It was shown years ago by Clerk-Maxwell that the ring of Saturn could not be a solid body, or it would fall on to the planet. It must be a collection of small satellites, revolving round the planet according to the law of satellites, viz., the outermost going most slowly, instead of most quickly if the ring were solid. The honour of confirming this mathematical demonstration by the direct evidence of the spectroscope belongs to Professor Keeler, the present Director of the Lick Observatory. A spectrum of Saturn and his ring obtained by him shows clearly the rotation of the planet according to

the law of solid bodies (as in the case of the Sun quoted above), and that of the ring as a collection of small particles; and this is a most beautiful and complete confirmation of this principle for observing "motion in the line of sight."

**Spectroscopic Binaries**

The principle having thus been confirmed experimentally by observation of bodies known to be rotating, and having confirmed the hypothesis of rotation in suspected cases, it was an obvious further step to detect rotation or revolution where it was not known previously to exist.

Double stars have been discovered by this method which cannot be seen directly as double stars even with the previous knowledge of their binary character. When two bodies revolving round each other are both bright (and not one bright and one dark, as in the case of Algol), the lines of the spectrum of one move to the right, while those of the other move to the left, so that what should be a single line appears double. A new method of finding double stars has thus come into existence, just as simple as the old method of looking at a star to see if it is double, viz., we look at the lines

in its spectrum and see whether they are double *at any time.*

**Concluding Remarks**

The foregoing account, however imperfect, will at least have made it clear that there are many new methods of work claiming attention at the present moment. A word or two may be added concerning the difficulties and dangers of changing old methods for new, even when the latter are obviously better. Astronomy occupies a peculiar position among the sciences owing to the vast number of observations of the same kind which it is necessary to make. All observers multiply their experiments to some extent—a careful physicist or chemist may repeat the same experiment twenty or even a hundred times, but no one save the astronomer deals in *millions* of measures of the same kind. It follows that he must not too readily change his instruments and methods when a long series of observations is in hand; he will do better to make up the tale of measures on one uniform system, even though this involves working for years on an old-fashioned plan, while his colleagues are reaping the benefits of modern appliances. Again, it is prudent to be sure before taking

up a new tool or new method, that it has reached a sufficiently stable form; it is aggravating to begin work and find within a year or two that a better plan still might have been adopted. Hence astronomers are slow to change their methods for good reasons, even in the presence of such obvious advantages as have been described.

# Section III

## MODERN RESULTS

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In the previous sections we have considered the new instruments recently put into the hands of astronomers, and the new methods which they have suggested. The interest of the general public is more generally manifested in the results of astronomical work than in the processes—in the actual discoveries rather than in the way they are made; and it will no doubt appear to some readers that the present section, dealing with modern discoveries and results, might with advantage have been expanded at the expense of the other two. It is, however, my object in the present work to lay more stress on the new methods than on the results already obtained, for two reasons: firstly, because discoveries are announced in other ways, while the important and interesting changes

in method are unnoticed; and, secondly, because the crop of results yielded up to the present, although of goodly size, is but a fraction of what we may expect in the near future, as the new tools grow more familiar and the new methods are better understood and perfected. The examples which follow, though fairly representative, are not intended as a complete collection even of specimens.

**Variation of Latitude**

Let us take, first, the movements of the poles on the surface of the Earth, or the "variation of latitude" as it is technically called. Though this discovery is in great part an outcome of old observations, it is also largely due to the almucantar; for it was his observations with this instrument which directed Mr. Chandler's attention to the subject, and ultimately led to the elucidation of a difficult problem. The history of the problem is curious. The question, Does the latitude of a given place vary? or, in other words, Does the North Pole, which our explorers go to seek, remain accurately in the same place on the Earth's surface? has been before the minds of astronomers for a long

time. It was soon recognised that if the North Pole does not remain quite stationary, its excursions are very small. It never wanders down into Europe, for instance, or we should have a different climate; its excursions cannot carry it very far on the way towards Europe, or the length of day and night would be sensibly affected. But are there any very minute excursions which might not be noticed in such ways as these, and which yet might be detected by astronomical measurements of great precision? The mathematician Euler investigated many years ago what would be the general character of such movements if they existed. He found that a rigid body like the Earth, which was spinning about an axis once a day with nearly complete steadiness, might have a slight "wabble," which would mean that the North Pole was in motion, but that, if so, the motion would complete a circuit every ten months. There seemed to be no doubt about this result, and astronomers examined their observations of latitude to see whether there was any change of ten months' period. *None was found,* although long series of observations were cut up into chapters of

**Euler's Ten Months' Period**

ten months and added together to magnify any possible small disturbance; and after several attempts of this kind, the question was regarded as settled in the negative. The North Pole did not move at all. So confident did astronomers feel on this point that when Mr. Chandler, who ultimately demonstrated the real facts so clearly, found an apparent movement of the Pole by observations with the almucantar in 1885, he himself thought he must have made some mistake, and did not follow up the matter. A year or two later, however, Dr. Küstner, of Berlin, published some observations made about the same time as Mr. Chandler's, which seemed also to show that the North Pole had moved in the same way. This independent confirmation aroused attention. In Germany a long series of special observations was initiated, by which it was hoped that the motion of the Pole would be detected and measured for a number of years. But this deferred the complete solution of the problem to a future time, when sufficient of these special observations should have been accumulated. Mr. Chandler, in America, adopted a different method. He felt that if the motion of the Pole was real, it

**Küstner's Observations**

ought to be just as plainly revealed by observations already made and published, as by those to be made in the future, for which we should have to wait. It was true that old observations had been already examined to no purpose; but possibly the examination had not been sufficiently thorough, especially as it had always proceeded on the assumption (following Euler's mathematics) that the disturbance, if any, must have a period of ten months. Mr. Chandler said, "Let us give up this limitation, and see whether the observations perhaps indicate some other period"; and the answer came almost at once, that they indicated a period of *fourteen months instead of ten.* Plainly, however, as the answer was written in the published observations, and plainly as Mr. Chandler indicated it, it was received with universal disbelief. The mathematicians replied that such a period was impossible: Euler's work had shown what period the motion must have, and any appearance of another period must be due to some error in the observations. Chandler replied to the effect that he did not care for Euler's mathematics: the observations plainly showed

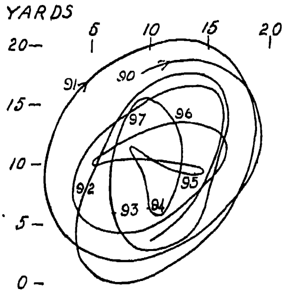
**Chandler's Fourteen Months' Period**

fourteen months, and if Euler said ten *he* must have made the mistake. I do not exaggerate the situation in the least: it was a deadlock: Chandler and observation against the whole weight of astronomical opinion and theory. No better instance could be given of the spirit which lately animated astronomy, which was referred to in the first section, amounting to a tacit assumption that certain problems had been worked out and settled, and had almost lost interest. But Chandler nobly stuck to his guns, and he had at last the satisfaction of capturing a mighty opponent in Simon Newcomb. It ultimately occurred to the latter that Euler had assumed the Earth to be a *rigid body*, and possibly it was this assumption which made the apparent discrepancy between theory and observation. A preliminary investigation convinced him that he had found the key to the rusty old lock; and since that time the reconciliation between theory and observation has been rapidly effected, though not without other pitched battles in which Chandler exhibited the same resolute bravery as before.

**Physical Explanation**

The Earth is not rigid; and when allowance

is made for its yielding to stress, we see that ten months is merely the *minimum* period in which it can wabble. The period is greater and greater according to the extent of yielding; so that Chandler, by his discovery of the true period of fourteen months, not only settled the question of variation of latitude, but




OBSERVED MOTION OF THE NORTH POLE, 1890–1898.

gave us a measure of the plasticity of the Earth. The amount of the wabble is very small. The North Pole is never a dozen yards away from its mean position; and its movements might almost be executed on the floor of this lecture hall.[1] There will thus not be any serious difficulty in identifying the spot if any of our brave explorers penetrate to

**Amount of Motion**

[1] At the Royal Institution.

the North Pole. To the dangers from cold and hunger there will *not* be added the mortification of finding, when the supposed North Pole is reached, that it has removed for the season to another locality; but at the same time its movements affect astronomical observations quite sensibly, and must be regularly taken account of in the future. The accompanying illustration shows the observed wanderings of the North Pole during several years. It will be seen that the motion is by no means simple, and the fourteen-months' period is not recognisable by a mere glance, even with this observed motion before us. The fact is that there are annual changes as well, mixed up with the others. The cause of these is not yet wholly understood; they are probably due to annual meteorological changes (such as the regular melting of the north polar ice every year, for instance), but the most important agent has not yet been identified. The discovery, like most others, by no means concludes our work in that particular direction; it rather opens up new fields of work. It is, however, a considerable triumph to have made such a step towards the solution of this old problem of the varia-

tion of latitude. For the complete solution we must wait till the end of time; but we may regard the provisional answer obtained by Mr. Chandler with considerable satisfaction.

**The Sun's Distance**

There is another problem of long standing of which a satisfactory provisional solution has recently been obtained; the question of the Sun's distance, or, as it is more technically called, of the Sun's "parallax." Enough has been said in the second section (see p. 106) to show the present state of our knowledge on this head. Thirty years ago the distance was uncertain by some millions of miles, a margin of error which it was hoped to materially reduce by the transits of Venus of 1874 and 1882. But this method failed. Fortunately, however, a better one was found almost immediately, and we may now give the Sun's distance as 92,874,000 miles, from Sir David Gill's beautiful observations, feeling some confidence that this is within 100,000 miles of the truth. This margin looks large in these actual figures, but it is equivalent to less than the thickness of a stump in a cricket pitch of twenty-two yards! The best

measures agree as well as measures of a man's height would if he alternately put on and off a pair of very thick socks!

It might seem that astronomers could therefore rest satisfied with their knowledge of the Sun's distance; but we hope to improve it still further during the present winter (1900–1), when the planet Eros is paying us a very neighbourly visit. He is such a tiny planet that even at his nearest approach he cannot be seen without a powerful telescope, but he will be the "observed of all observers" for several months.

**Habitability of the Planets**

The mention of our neighbours naturally suggests the old question of the existence of life on other planets, which is of such paramount interest to all of us. Have the large telescopes recently erected brought us any nearer to the solution of this problem? I fear that what there is to report will be a disappointment. The recent increase in size of telescopes, and even their installation in vastly better climates for observation, is quite inadequate to take us any nearer such knowledge.

**Canals in Mars**

We have heard a good deal in late years of the canals in Mars; and there is no doubt at all

that certain straight markings on the planet's surface have been detected. Many of us have sufficient faith in that wonderful observer Schiaparelli, to believe that these are occasionally seen double. But as regards the interpretation of such markings,—the notion that because they are called canals it is implied that there are inhabitants in Mars who have dug them for irrigation purposes,—we must exercise much more caution.

To realize the value of our information, consider first how much farther away Mars is than the Moon—about 200 times at least, and generally much more. Now 200 is about the magnifying power of a good telescope, that is to say, the magnifying power which can be used with advantage. It follows, then, that whatever a fair telescope enables us to see on Mars could be seen on the Moon with the naked eye; and it may be added that whatever the largest telescope in existence would enable us to see on Mars, could be seen on the Moon with a pocket opera-glass: for our gain from the recent increase in size of telescopes is well within that represented by a small opera-glass as compared with the eye. Hence,

let any one look at the Moon, with the naked eye, or even with a small opera-glass,[1] for traces of canals or other signs of life of any kind, and he will begin to understand the caution which must be exercised in drawing conclusions, however attractive, as to the habitability of the planets. We want, in fact, an increase of our optical resources by a thousand times at least to get any satisfactory intelligence of this kind: whereas the advances of the last century would be represented by a factor not greater than 10, and there seems no chance at present of our getting to 100: we might manage 20, perhaps, by slow and costly advances, but 100 seems impossible.

A brief statement, and especially a numerical statement, of this kind should not be criticised too closely in detail; but it may be accepted unhesitatingly as giving a general idea of the situation. The usual numerical form of statement is to give the apparent distance at which

[1] Professor E. E. Barnard, who has had probably more experience of the largest telescopes in favourable conditions than any one, is of opinion that the naked eye view of the Moon better represents the view of Mars through the best telescope. He kindly gave me this opinion, after a little consideration, in reply to a definite question.

a body is viewed by a telescope. In the advertisements of the great telescope built for the Paris Exposition this year, the phrase " La lune à un mètre " was used. This obvious extravagance is thus corrected in the guide to the Palais d'Optique as follows:— **"La Lune à un Mètre"**

"La lune n'est pas rapprochée à un mètre mais à environ 70 kilomètres, ce qui est déjà fort joli."

To bring the Moon within 70 kilometres, instead of its actual 380,000, simply means that a magnifying power of $\frac{380000}{70}$ or 5,400 can be used on the telescope. Now, there is no physical impossibility in putting a magnifying power of this amount on this telescope or any other—it is done by putting in a suitable eyepiece, and if none suitable is at hand it is only a matter of some shillings to make one. But the real question is whether this power can be used *with advantage.* A similar case arises in the enlargement of a miniature portrait: by a moderate enlargement we see the features more clearly, but after a certain point we lose rather than gain by further enlargement. We cannot, for instance, by enlarging 10,000 times get a picture on **Limitation of Magnifying Power**

which the hairs of the head are all shown separately; what would be shown separately would be the grains of the film or paper forming the miniature portrait, and the huge image might be quite unrecognisable. If the original miniature was very clearly defined, we could carry the process of enlargement further than if it were not; but in all cases there would come a somewhat indefinite limit beyond which we could not enlarge with advantage.

With a telescope, the big lens or mirror forms a miniature image, and the eyepiece enlarges or magnifies it, to an extent controlled by the focal length chosen for the eyepiece. The larger the lens or mirror, the more clearly defined is the miniature image, and therefore the more we can magnify it with advantage: but there is in all cases a limit beyond which it is futile to go. This limit is not very definite and is differently assigned by different observers. It depends, too, on the climate in which the telescope is used. Whether a power of 5,400 can be used on the big Paris telescope is doubtful; at the present moment the big lens for visual observations is not made, and so no one can speak with certainty. As-

suming for a moment that it is possible, *and that our atmosphere did not exist to trouble the image with its air currents*, we should be able to study the surface of the Moon as we should a map, on a scale of one inch to four miles, held at a foot from the eye. A glance at a bicycle map will give some notion of the apparent size of objects under these conditions. We should not be able to see human beings, but we could recognise towns, villages, and roads, and might infer the existence of life from changes in these products of activity.

**Changes in Moon**

In spite of difficulties such as these it is probable that we can detect, and have detected, changes going on in the Moon. The text-books usually call the Moon "dead," and say that there is no sign of change; but the following pregnant remark recently made[1] by Professor W. H. Pickering is well worthy of notice.

"In reviewing the history of Selenography, one must be impressed by the singular fact that, while most of the astronomers who have made a special study of the Moon, such as Schroeter, Maedler, Schmidt, Webb, Neison and Elger, have all believed that its surface

[1] *Harvard Annals*, vol. xxxii. p. 175.

was still subject to changes readily visible from the Earth, the great majority of astronomers, who have paid little attention to the subject, have quite as strenuously denied the existence of such changes."

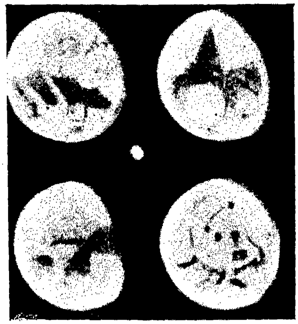
The consensus of expert testimony in favour of change is indeed remarkable. What then is the reason of the ordinary statement? It is doubtless due to the fact that there is known to be no appreciable[1] atmosphere on the Moon such as could support any life that we know of; and the changes noticed are therefore not considered significant of life. It is indeed quite likely that they are due to volcanic activity on the Moon: Professor W. H. Pickering considers that the lunar volcanoes are more active than those on the Earth. But again it is quite difficult to be certain even of these changes. The same object on the Moon's surface looks so different at different times, from the varying illumination of the Sun, that it needs very careful records indeed to make certain that a change is real.

[1] In the volume just referred to, a good case is made out for an extremely tenuous atmosphere, especially on the side of the Moon illuminated by the Sun.

With photography now to help us (and M. Loewy is devoting himself specially to the photography of the Moon with the large equatorial *coudé* at Paris ; but he gets very few plates during the year which satisfy his exacting requirements) we shall presently attain greater certainty on this point. But meanwhile it is well worthy of note that those who have studied the Moon all agree that changes are going on, while those who have studied Mars do not by any means agree that the canals are double. *It is far better established that changes are going on in the Moon than that the canals in Mars are double.* In saying this I do not wish to disparage the keen sight of Schiaparelli : but others do not always confirm his observations. Look, for instance, at the drawings of Mars in the illustration. They represent all that Professor Barnard, one of the keenest sighted observers, could see with one of the largest telescopes under the very best conditions. The diameter of the disc of Mars is more than 4,000 miles, which gives the scale of the map. With these pictures and this information any one of intelligence is in practically the same position as regards drawing conclusions concerning life on the planet.

**What ca[n] be seen [of] Mars**

Does it seem likely that any ingenuity of interpretation will avail anything? No: if any one could construct, *and use with advantage*, a telescope one hundred times larger than the largest in use—say three miles long, with a lens a quarter of a mile in diameter—we *might*



FOUR DRAWINGS OF MARS BY BARNARD.

learn something about the habitability of the planets; but while we are still in difficulties about telescopes measured in yards, miles or furlongs seem out of the question.

**Real Work with large Telescopes**

But it must not be thought that the performances of large telescopes have been disappointing because they have told us nothing of the habitability of the planets. We might as well be disappointed with the electric light

for not having given us perpetual daylight. All that astronomers could fairly expect from large telescopes was that they should render certain difficult observations rather easier, and render possible some that had been previously just beyond the limit of possibility; and such expectations they have amply fulfilled. The second test is the more definite; and it is not a little remarkable how immediately some of the large telescopes have justified their construction by making a discovery early in their history.

**Discovery of Satellites of Mars**

Thus it was very soon after the erection of the big Washington refractor that Asaph Hall discovered with it the two minute satellites of Mars. Was this a genuine instance of an increase in size of the telescope rendering a previously impossible observation just possible? The answer is a little uncertain. There is no impossibility (though considerable difficulty) in seeing these satellites with smaller instruments, *now that they have been discovered*; but the words in italics are important. There is a vast difference between seeing a thing when it is known where to look for it, and actually seeing it for the first time; and so it is not

unfair to say that an instrument which can now see the satellites could not have discovered them. In this sense the Washington refractor rendered possible what was previously just outside the limits; and if an accident had destroyed it immediately afterwards, the cost and labour of erecting it would have still been amply repaid.

**Fifth Satellite of Jupiter**

So too with the great Lick telescope: we may take the discovery of the minute fifth satellite to Jupiter by Barnard, in 1892 (an object so minute that only the largest telescopes on favourable nights can reveal it even when its place is known), as an achievement fully justifying the existence of the telescope if it had done no more. These new satellites are not mere tiresome additions to the numerous family of the solar system, as the impatient might call many of the 400 or 500 minor planets; in both cases they present new features which seem likely to guide us in unravelling the past history of the solar system.

**Capella as a Binary**

Another instance of rather a different kind may be given. It has been explained (see p. 192) that a star may be found to be "double" by

means of the spectroscope, although no existing telescope can separate the components. We see the lines in the spectrum of the star double, which means that there must be two bodies sending light to us, one of which is approaching and the other receding; and we infer that there are two stars revolving round each other, though in the most powerful telescope we can only see one point of light. Until the present year this inference had never been confirmed by actual observation at the telescope. This did not mean that there was any flaw in the argument, but simply that the revolving stars were always so close together that it was beyond the power of our best telescopes to distinguish them apart: and so, although confidence in the inference was not shaken, the interest in finding a particular case where it could be independently confirmed grew naturally keener. The large refractor at Greenwich has been the first to detect such an instance, in the well-known star Capella. Credit is not due altogether to the telescope; much is due to Mr. Newall, of Cambridge, who shares with Professor Campbell, of the Lick Observatory, the honour of making spectroscopic observa-

tions which showed Capella to be double, an who suggested, from a study of these observa tions and of the parallax found by Dr. Elkin, c Yale, that here might at last be a case wher the duplicity of the star could be actuall detected with a powerful telescope. Th suggestion was not lost on the Greenwic observers: they applied themselves diligentl to watch Capella, and found that althoug they could not actually see two stars sepa rated, they could detect an elongation of th image in a definite direction. By repeate observations they found that this directio changed, gradually performing a complet circuit in the time in which it was indicate by the spectroscopic observations that th stars should be revolving round one another and the chain of evidence was thus completed

This most interesting discovery, while no entirely due to a large telescope, could no have been made without one, and it is not un fair to include it in our category of justifica tions for the construction of large instruments

**Extended Observations of Comets**

One more instance may be given. Ou observations of comets have been usuall confined to the brief period when they ar

near the Sun and near us, which represents only a very small portion of their whole orbits. Such observations are sufficient to enable us to predict the return of certain comets, even though we may lose sight of them for many years; but what happens to them in the meantime we have not been able to say, for ordinary telescopes completely lose sight of them. With the Lick telescope and the Californian climate Barnard has been able to follow a comet so far on its outward journey, so long after other telescopes had lost it, as to raise hopes that we may perhaps soon be able to follow some comet all round its orbit. If so, we may learn something more of the nature of these mysterious visitors. This is as yet only a possibility; still, it is by following up such possibilities that advances in knowledge are made, and without big telescopes we should not have even the possibility.

**Results du to Photography**

One of the facts which has most arrested attention of late years is that objects can be photographed which cannot be seen. Since the discovery of the Röntgen rays, by which photography triumphs over the eye in quite a novel manner, it seems less extraordinary that

we should be able to detect by the sensitive plate light that is merely *too faint* for the eye; but not many years ago this was a new and remarkable advance. It really dates from the discovery of the dry plate, which has allowed us to prolong exposures indefinitely; so that by giving longer and longer exposures of a photographic plate at the end of a telescope pointed to any region of sky, we find that we get on the plate fainter and fainter stars; and we can continue the process until we have certainly got stars on the plate (and nebulæ too), which the eye cannot see with the best telescope; and even then we can go on further. The only limit to the time of exposure which has yet been reached is that assigned by the patience (or impatience) of the observer. If the night clouds over, or the dawn comes, he can shut up the camera and wait till the next fine night, when he can open it again and continue the exposure; and so on for months or years. Up to the present I do not know of any photographs having been taken with a cumulative exposure of more than fifty hours; but there is no particular reason for stopping there, and doubtless we shall have longer ones in the future.

Stars which cannot be seen

These discoveries, however, of stars which have not previously been seen because they are too faint for the eye even with the largest telescope, though they are in a very real sense discoveries, are not of any very great importance, for the reason that the vast majority of the stars are of no special interest. They are to be found in the same position, looking exactly the same for centuries to come as in centuries past. They are not quite so uninteresting as Euclid's "points"; for though they have no "parts" (so far as we can see at such a great distance) they have "magnitude," in the astronomical sense of brightness. We have good reason for assuming them to be bodies as large as our Sun—some of them much larger; but they are so far off that it is quite hopeless to detect any dimensions, as hopeless as to see objects about the size of human beings in the Moon. In the largest telescope they are mere points of light; and their position and brightness remain nearly constant in the great majority of cases. The number of "variable stars" known is not twenty in a million of those which can be seen, and these must be excepted; there are also stars which are of interest because they are double, are compara-

tively near us or are in comparatively rapid motion, or have a peculiar spectrum. Excepting all these, which form only a minute fraction of the total number, we may say that the average star is of no particular interest, and hence the occupation of discovering new ones is profitable only so far as that among the number there may by chance be one of these exceptional cases—a new variable or a new binary; but then the vast number of stars easily visible in any telescope have only as yet been searched very inadequately for such objects. Hence, though the discovery of new stars is undoubtedly one of the achievements of the photographic dry plate it cannot be regarded as one of its greatest triumphs.

**New Asteroids**

In thus disparaging the importance of anything new, however, we are always likely to be proved at fault; and in a closely allied field of work we have recently had a conspicuous instance of the danger of such judgments. It has been already remarked that photography has been applied with great success to the discovery of minor planets. The old laborious process of watching each star, to see whether it was moving, has been superseded by the

simple expedient of exposing a photographic plate, so that any planet in the field may betray itself by its little trail. It was mentioned that the number of these bodies had accordingly rapidly increased, and interest in them had begun to flag; for most of them are just as uninteresting as stars. It is true they are in motion while the stars remain fixed, but this adds to the difficulty of keeping track of them rather than to our interest in them; for beyond the fact that they are moving round the Sun under the well-known law of attraction, there is no special feature in their movements. And we cannot see any detail on their surfaces, not because they are far away like the stars, but because they are actually so small—the surface of most of them would not be so large as that of the British Isles. Hence, the occupation of finding new minor planets was beginning to be regarded with disfavour, as adding a new burden without any compensating advantage. But in the autumn of 1898 one of these little bodies was discovered worth hundreds of others put together; for it turns out to be our nearest neighbour in the solar system, after the Moon and Venus. The importance

of this new planet Eros has been already explained (see p. 107) ; it makes periodical near approaches to the Earth, and on these occasions we hope to obtain an accurate measure of the Sun's distance. The discovery is mentioned here as a reminder that there is a danger in regarding any field of work as unprofitable. We may find a rare gem in what is supposed to be a pan of ordinary gravel.

**New Satellite of Saturn**

A few months ago photography discovered its first satellite—a ninth satellite of Saturn. All previous discoveries were made by eye, including those of the other eight satellites to Saturn; but on developing a series of photographs of the planet and its neighbourhood, Professor W. H. Pickering (brother of E. C. Pickering) found traces of an object, which occupied different positions on different dates, in the manner of a satellite. There seems to be sufficient evidence of the existence of this new satellite; but as yet it has not been detected visually even with the largest telescope; and it has not been photographed again, for specially good conditions are required, and they did not recur in time. For further confirmation we await a favourable opportunity.

**New Comets**

It was mentioned in the last section that new comets had been discovered by photography; and beyond this general statement no particular discovery calls for remark. If among the number had occurred one of those exceptional comets which becomes a striking object in the heavens easily visible to the naked eye, like those of 1858 and 1882, we might have dwelt a little on the discovery. But we have had none of these for some time (and in answer to the inquiry sometimes put, "When are we going to have another bright comet?" I may reply, "We cannot tell in the very least: there is no regular recurrence of such which enables predictions to be made"); and both photographic discoveries and those made in the old-fashioned way (which are still the great majority), have been limited to those small comets which are only visible in a telescope and do not interest the public. Of these there are generally half a dozen found every year.

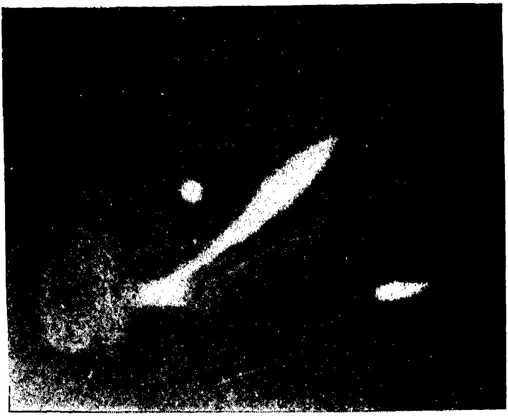
**Comets' Tails**

Photography has, however, told us a good deal of which we were previously quite ignorant—the striking changes in comets' tails. These are subject to sudden convul-

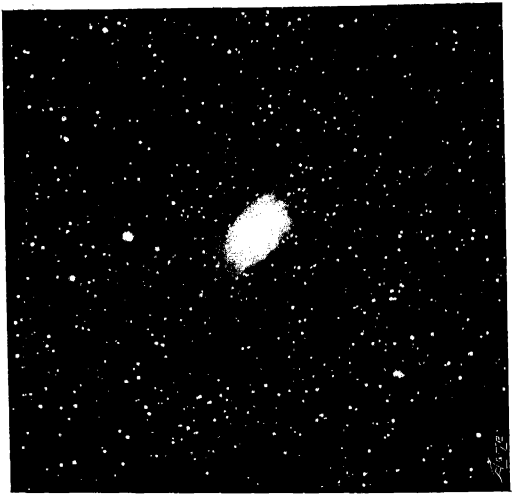
sions which shatter them into extraordinary forms, the origin of which is not yet understood. Professor Barnard is fond of telling the story of a lady to whom he showed one of his photographs of a comet, with the tail in a peculiarly ragged and disreputable condition: whereon she remarked, "Why! that comet looks as if it had been out all night!"

**Forms of Nebulæ**

The reason why such changes of form were little known before the days of the dry-plate is that it is very difficult to get an idea of the form of such objects with the visual telescope: firstly, because they are faint, and secondly, because so little of them can be seen at one time, the field of an eyepiece being small. This point has been already referred to (see p. 138); and we now pass to an illustration of its importance, not in the case of a comet, but in that of a nebula, which is in some respects an object of the same kind. We shall presently refer to the discovery of new nebulæ by photography; but the present example is intended to show how much photography has taught us about an old and well-known nebula—that in Andromeda. It is a



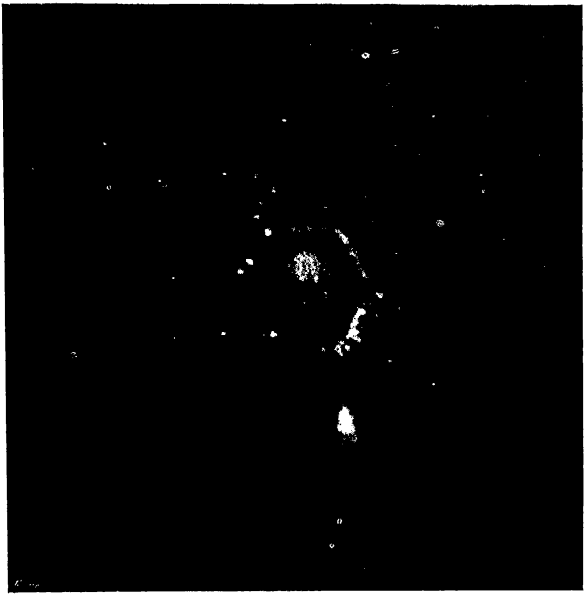
Trouvelot's Drawing.



Roberts' Photograph.

THE ANDROMEDA NEBULA.

DE LA RUE'S DRAWING OF SATURN.



THE SPIRAL NEBULA IN CANES VENATICI (LICK OBSERVATORY).

large bright object, visible even to the naked eye, and is a tolerably easy object to draw at the telescope. But the drawings, even of the best observers, left us in ignorance of an essential feature of the object, which was revealed directly it was photographed. Look first at the drawing shown alongside the photograph, and notice specially the two dark rifts. The draughtsman has made them *straight*, whereas it is seen in the photograph that they are slightly but sensibly curved. The draughtsman is not very far wrong, but just so far as to miss the whole point of the formation which we see so admirably in the photograph. The rifts are really the separation between the central nebula and a ring thrown off from it, seen in perspective; and we see here actually in the sky the state of things which Laplace suggested in his famous Nebular Hypothesis—a central nebula, which in its rotation throws off a series of rings, some of which break up to form satellites. There are two satellites already formed, and others are in course of formation. The system closely resembles (except in crispness of outline) that of the planet Saturn, as we see by the drawing annexed, which is an actual drawing

**The Nebular Hypothesis confirmed**

made by Dr. De la Rue in 1852, not altered in any way beyond being turned to an orientation resembling that of the nebula. By a curious coincidence two satellites of Saturn are shown by Dr. De la Rue in somewhat the same positions relative to the central body as the satellites of the nebula —but this is merely a coincidence of detail. The general evolution of such a system, with rings and satellites thrown off by the rotation of a central nebula, all becoming more definite by condensation, was long ago suggested by Laplace as the possible history of our solar system; but we had no direct evidence of the occurrence of such phenomena, beyond that indicated by the system of Saturn, until this nebula in Andromeda was photographed. The first photograph to show the true nature of the nebular system, which was taken by Dr. Isaac Roberts, thus stands in the same relation to Laplace's nebular hypothesis as do some of Galileo's first glances through his telescope to the Copernican theory. Copernicus made the Moon a satellite to the Earth; but no confirmation was forthcoming in the shape of similar satellites to other planets, until Galileo looked through his

"optic tube" at Jupiter, and discovered satellites to that planet, and then the idea of the Moon being a satellite was no longer strange. On the Copernican theory Mercury and Venus ought to exhibit phases like the Moon; but there was no evidence of this until Galileo looked through his telescope and saw them. The invention of the telescope supplied just the evidence wanted for the Copernican system; and the invention of the dry-plate has put in evidence Laplace's nebular hypothesis as an actual fact. Many beautiful pictures have been taken of other nebulæ, and some of them are very similar to the Andromeda nebula: others show a spiral structure (see illustration), which suggests a rather different historical development. The full significance of all the information thus acquired is scarcely yet realized, but it all tends to throw light on the history of the universe. The information is accumulating on our hands so rapidly that there has been no time to arrange and study it properly; but it seems quite probable that when the forms are classified, we shall learn something new about the history of stellar systems. And besides the study of individual forms there is that of the distribution of

**Spiral Nebulæ**

**Distribution of Nebulæ**

nebulæ over the sky. They do not occur in all quarters of the sky equally, but seem specially to avoid the Milky Way: while, on the other hand, star-clusters congregate in the Milky Way. What is the meaning of this? At present we cannot say: I do not think that even a good guess has been made; but it is clear that the more we learn of nebulæ and clusters the more likely we are to interpret this most important fact.

**New Nebulæ**

It has been said that the information is accumulating rapidly; and precision can be given to this statement by a few figures. Professor J. E. Keeler,[1] the Director of the Lick Observatory, is finding on his photographic plates about three *new* nebulæ per square degree, which would lead him to expect about 120,000 in the whole sky! It will be obvious that to arrange and digest such a mass of information is not a light matter.

The instrument used by Professor Keeler is a 3-foot reflector, the same instrument with which the pioneer in this work, Dr. A. A.

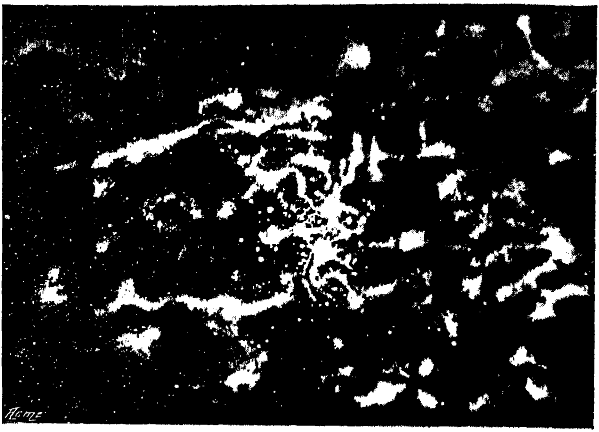
[1] While these sheets were in the press, the sad intelligence reached England of the sudden death of Professor Keeler.

Common, of Ealing, took his photographs of the Orion nebula in 1882–3, for which the Royal Astronomical Society awarded him its gold medal. His instrument was afterwards purchased by Mr. E. Crossley, of Halifax, who subsequently presented it to the Lick Observatory. Here, in the clear air of Mount Hamilton, and in the able hands of Professor Keeler, it is doing the magnificent work above mentioned. The reflector seems to be specially fitted for the work of photographing nebulæ. Dr. Isaac Roberts, to whom fell the honour of taking the first photograph showing the character of the Andromeda nebula, and who has published two volumes of beautiful photographs of nebulæ, has used a reflector throughout.

**Diffused Nebulæ**

But there is another kind of nebula for which the reflector is comparatively useless, and the portrait-lens is the proper instrument. The above objects, which are being photographed at the Lick Observatory by the thousand, are all small and well defined, or at least well separated from each other. It has, however, gradually become apparent that there are whole regions of the sky over which extremely

faint nebulous matter extends, with marvellous ramifications and interlacings. These objects are too faint to be seen by the eye even with the best telescopes, but if a long exposure is given with a powerful portrait-lens they may be photographed. Look, for instance, at the



DIFFUSED NEBULA NEAR THE PLEIADES (MAX WOLF).

picture of the Pleiades shown in the illustration. With the largest telescope little more can be seen in this region than stars, which the picture shows as round dots. A few wisps of nebula were detected in the very middle of the picture some years ago, but so little that

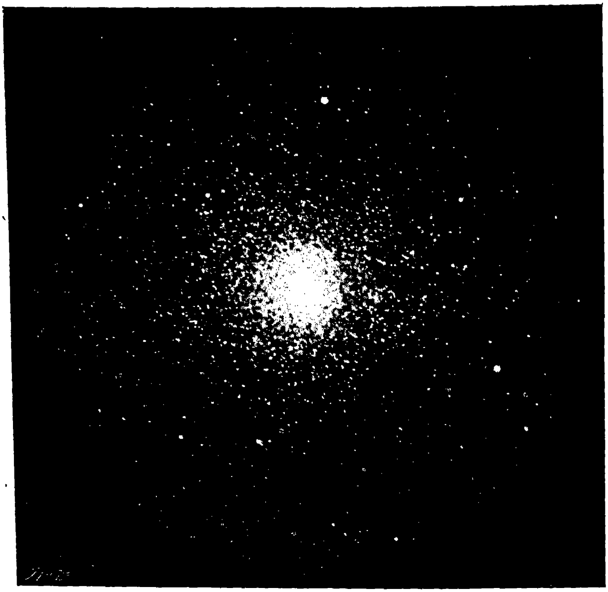
we may neglect it entirely, and say that the eye can only see the stars. The picture shows us what a portrait-lens can reveal to us—a revelation indeed! All over the region is a nebulous structure to which there seems no limit. We begin to wonder whether there is not an invisible veil of nebula over the whole sky, which would betray itself with a long enough exposure. Here, again, we are getting information which we have only had time as yet to marvel at—not to interpret.

The actual picture is drawn from his photographs by Dr. Max Wolf, of Heidelberg; but it was Professor Barnard who first drew attention to these diffused nebulæ by his beautiful pictures taken at the Lick Observatory with a portrait-lens (the Willard lens), and even with a cheap magic-lantern lens. These first revealed the large diffused nebulæ of which that round the Pleiades is a striking illustration. Valuable work at an observatory is not always done with its largest telescope. The achievements of the Crossley reflector and of the Willard lens—even of a small lantern lens, in Barnard's skilful hands—range worthily alongside those of the giant refractor, which many

people regard as essentially constituting the Lick Observatory.

**Variables in Star-Clusters**

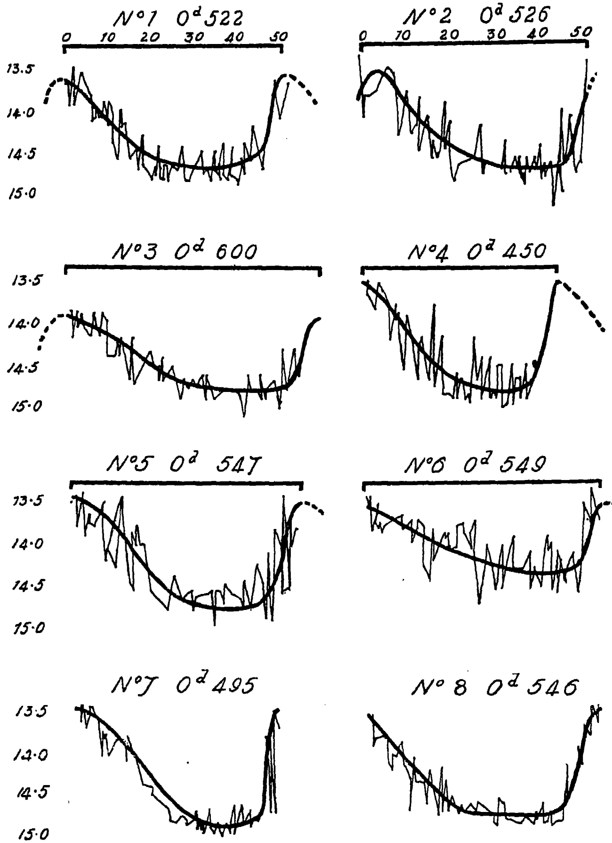
It was mentioned above, that while the nebulæ seem to avoid the Milky Way, star-clusters



THE CLUSTER ω CENTAURI.

seem to prefer it. A notable discovery about star-clusters has been made by Mr. S. I. Bailey, of Harvard, viz., that a very large proportion of the stars in them are variable. In one

ster 85 stars are variable out of 900, which
a very large proportion compared with the
inary sky. Moreover, the variations of light
w a striking similarity: each goes through
egular cycle of changes in something like
lve hours, some have a period as short as
hours, and some as long as fourteen, the
rage being about twelve and a half. Each
ins by a gradual diminution of light to
ut half its maximum, remains at half-
ghtness for a sensible time, and then
ings back again to full brightness. The
denness of the return to full brightness is
te remarkable, as may be seen from the
ves for some of these stars shown in the
stration. Now, here again is a striking
which as yet we cannot explain. What
he cause of this peculiar variation of light?
he case of the variable star Algol, it is toler-
certain that the light-variation is due to
existence of a dark body circulating round a
ht one, and regularly eclipsing it at inter-
. Not only does this hypothesis fit the
s, but there is independent spectroscopic
lence of its truth. But neither this nor
allied supposition will fit the light curves
these cluster-variables. We have here




LIGHT CURVES FOR 8 OUT OF 85 VARIABLES IN THE CLUSTER MESSIER 5, AS DETERMINED BY S. I. BAILEY, OF HARVARD, 1899.

another discovery of which we are not as yet able to take full advantage.

**Tennyson's Astronomy**

Although this is all so new, it is at the same time curiously old. In 1835, Tennyson wrote in his "Palace of Art" the following stanzas. They were omitted later because he thought that the poem was "too full,"—so his son tells us in the *Life of Tennyson*, from which I have taken the stanzas. In the centre of the four quadrangles of the palace is a tower, and

"Hither, when all the deep unsounded skies
Shudder'd with silent stars, she clomb,
And as with optic glasses her keen eyes
Pierced thro' the mystic dome.

Regions of lucid matter taking forms,
Brushes of fire, hazy gleams,
Clusters and beds of worlds, and bee-like swarms
Of suns and starry streams.

She saw the snowy poles and moons of Mars,
That mystic field of drifted light
In mid Orion, and the married stars."

**The Spectroscope**

Enormous additions to our knowledge have been made by the spectroscope, but many of them do not admit of a brief and crisp state-

ment as definite discoveries or results. When the spectroscope was turned, in 1864, for the first time to one of the nebulæ, a definite step in advance was made. It was seen at a glance that the spectrum consisted of a few distinct bright lines, and hence that the nebula was a mass of glowing gas. Before this it had been doubtful whether it might not be merely a star-cluster, the stars so small and close together as to be inseparable in the best instruments. Sir William Huggins knew before he put his eye to the instrument, on August 29, 1864, that he was about to get a definite answer to this question, and felt an intense thrill of excitement accordingly. But such opportunities are comparatively rare, even in the early days of a new instrument. It is a commoner experience to attain a definite result only after accumulating masses of observations, and studying them carefully. Much of the work done by the spectroscope has consisted in obtaining pictures of the spectra of different stars for comparison and classification; and as time goes on, the crispness and definiteness of the early results is lessened rather than increased.

**Classification of the Stars**

For instance, the stars were divided at first into four or five well-defined classes or types, according to their spectra; but with more information the divisions between the classes have been broken down by the addition of intermediate types; and now, although we can form a long string of spectra in regular order, so that the main features change gradually and regularly as we pass along the series, we can only cut up the series into sections quite arbitrarily, as the milestones divide up a road, to use the favourite illustration of an eminent spectroscopist. Again, it does not seem certain that the present arrangement can be regarded as final. Various classifications have been proposed by different workers, but for the moment we will consider two specially: those of Professor Pickering, of Harvard, and of Mr. McClean, of Tunbridge Wells. Both these earnest workers have surveyed the whole sky,—north and south hemispheres—and both used an object-glass prism to do the work; but in other respects they have differed. Professor Pickering, the head of a great observatory, which has the exceptional feature of a branch establishment in Peru, so as to command both hemispheres, naturally worked through his

**The Harvard Survey**

assistants. The prism was of small angle, so that the spectra were short, and showed main features rather than great detail; but a large number of stars were photographed on each plate, so that altogether the spectra of an immense number of stars were obtained. These were examined and classified by a comparatively rapid process.

**McClean's Survey**

Mr. McClean did the whole of the work himself with his own eyes and hands: the northern half in his own private observatory at Tunbridge Wells, the southern during a six months' visit to Sir David Gill at the Cape of Good Hope Observatory. He only photographed the spectrum of one star at a time, limiting himself to the brighter stars. But he was thus enabled to use a prism of larger angle and get much greater dispersion. His spectra are given in considerable detail.

On comparing the two sets of results it is sometimes found that a star which has been assigned by Professor Pickering, guided by the main features of the spectrum, to one place in the series, is found, when the details shown on Mr. McClean's photographs are studied, to belong really to quite a different

place: the greater dispersion shows cause for some rearrangement of the classification. It seems possible, then, that a future advance in the same direction may call for further modifications.

**Temperature of Stars**

Yet again, there is some doubt about the general interpretation of the classification or series. It may be that the stars so classified are arranged according to their temperature. Sir Norman Lockyer is a strong advocate of this view, and points out very confidently which are the hottest stars. On the other hand, Sir William Huggins has shown recently that certain features in a spectrum need not be due, as had been supposed, to the high temperature of the star, but to the *quantity* of certain substances present; and we shall see in the next few paragraphs how seriously different may be the quantitative distribution of an element in different bodies.

For such reasons as these much of the most interesting part of spectroscopic work cannot be stated in the form of definite results. But there are some notable exceptions, and to mention one or two of these will give a general idea of the importance of the work,

leaving completer knowledge to be sought elsewhere.

**Discovery of Oxygen in the Stars**

A good concrete example is afforded by the recent discovery of oxygen in the stars. It was a strange thing that oxygen, so vitally important to us, and so lavishly distributed over our globe, could never be detected (by the spectroscope) in any other celestial body. Its lines have been looked for persistently, but without success, in the spectrum of the Sun; twenty years ago Dr. Draper thought he had found them there, but it was proved to be a mistake. The veteran, M. Janssen (who came out of Paris in a balloon during the siege of 1870, because he wanted to observe the total solar eclipse in that year, and who is responsible for the sensational project of building an observatory on the top of Mont Blanc), has spent a considerable portion of his life in trying to detect oxygen in the Sun, or prove its non-existence. The particular difficulty with which he has done battle is this: since sunlight comes to us through our atmosphere which contains oxygen, its spectrum naturally contains traces of the oxygen lines; the question is, are these traces *exactly* such as

are due to our atmosphere, or is there an excess which must be due to the Sun? M. Janssen has arranged striking experiments to test this point. He has for observatory what was once an imperial palace, reduced to ruins in the war of 1870. The imperial stables, however, are not damaged, and the stalls for the imperial horses form efficient supports for some very long tubes into which M. Janssen compresses oxygen equivalent to what is between us and the Sun. Looking then through such a tube, at a source of light known not to contain oxygen, he sees the traces introduced by passage through the oxygen in the tube, and can accordingly estimate the allowance to be made in observing the Sun.

Another and more sensational experiment was to observe the lights on the Eiffel Tower in a similar manner; for between Meudon and the Eiffel Tower there is just about as much oxygen (compressed as it is close to the Earth's surface) as between Meudon and the Sun.

And finally, by getting to the top of Mont Blanc he diminishes the amount of oxygen

between him and the Sun in a known propc
tion, and can draw conclusions from the effe
on the solar spectrum.

The conclusions from all these experimen
are, however, so far negative—oxygen cann
be detected in the Sun. Since the oth
planets only shine to us reflected sunlight, v
cannot say whether they have oxygen or no
we *presume* so, but we have no direct evidenc
and the uncertainty has an important bearin
on the possibilities of life on other planet
Nor was oxygen discernible in any of tl
stars. To all appearance we might be tl
only body in the universe possessing oxyge
at all; it seemed a possible but extraordinar
isolation that our very life-giving elemer
should not be shared by any other celestia
body.

This absolute isolation has been remove
within the last two years by Mr. McClear
In the course of his survey above referred to
he found certain lines in the spectrum of th
star $\beta$ Crucis, which did not correspond wit
those of any other star, or element alread
known in the stars. On search being mad
they were found to be lines due to oxyger

The discovery has been confirmed by others, and the lines found also in other stars, so that we are no longer peculiar, though possibly specially favoured.

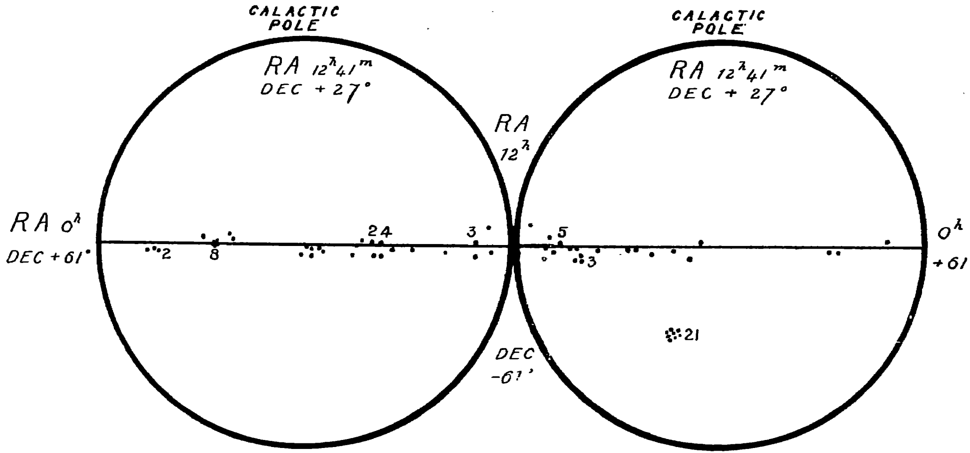
**Discovery of Helium on Earth**

On the other hand, an element of which we have only a minute quantity, and until a few years ago were believed not to possess at all, seems to be of the first importance in the sidereal universe—the element helium. It was known to us until recently only by a single line in the spectrum of the Sun; an important line which did not occur in the spectrum of any known substance. A few years ago, soon after the discovery by Lord Rayleigh and Professor Ramsay of the new gas argon in our air, and during experiments undertaken with the view of obtaining argon, Professor Ramsay discovered the element helium in the rare mineral cleveite. The whole of its spectrum was thus identified—not merely the one line; and it was then seen that this element occurred in a very large number of stars, and the progressive variation of the intensity of the lines representing it was one of the chief features guiding the arrangement of spectra in series. Helium is in some intimate way

connected with the evolutional history of the stars; and that we have so very little of it is a very significant fact, of which we do not yet know the full meaning.

**Distribution of the Wolf-Rayet Stars**

Let us now turn to a different kind of discovery. In arranging or classifying the stars by their spectra, there is a peculiar class which does not readily take its place in the series. These are usually called the Wolf-Rayet stars, from the names of the two astronomers who first noticed them in 1867: they have bright lines in their spectra, and are probably not so much stars as nebulæ. More than 100 of these stars are now known, and they nearly all congregate close to the Milky Way, the only exception being one group in what may be called a detached portion of the Milky Way. The discovery of many of these stars, and the establishment of this fact, are some of the many achievements of the Harvard University Observatory under its present able Director, to whom I am indebted for the information shown in the diagram. This is another remarkable fact which seems to show that the Milky Way is in some sense the backbone of our sidereal system. But in what




DISTRIBUTION OF THE WOLF-RAYET STARS.

(The straight line runs through the middle of the Milky Way. Where the dots are too numerous to be shown separately, the number is given in figures.)

sense? The ordinary nebulæ avoid it, the star-clusters and these Wolf-Rayet stars are found there and not elsewhere. What general idea of the stellar universe will co-ordinate these facts? It has been suggested that the whole system of the stars is spinning round an axis, of which the Milky Way represents the Equator, and that one class of bodies is driven away from the axis, while another class is sucked towards it, much as when a cup of tea is stirred heavy particles are flung to the sides, and light ones drawn into the middle. There may, of course, be such a rotation, even comparatively rapid, without our being conscious of it. We can detect the rotation of the Earth, because the stars do not partake in it and we see them sweep past; but if all the universe is rotating, where are the landmarks by which to observe it? The only signs may be just these signs we have noticed. But the suggestion is a vague and difficult one; it does not take us appreciably further than the hope that the real explanation may not be very far away—it may be that some happy thought will reveal it almost any day. We know too little as yet; we know, for instance, nothing of the distances of the nebulæ, or

their changes in form. They are certainly so far away that their parallax is very small, and even vast changes might appear minute to us; and until recently such changes were hopelessly masked by the uncertainties of drawing. Now, with photography to help, we may hope that the comparison of photographs taken at long intervals may tell us something of the changes, though as yet this page in our observation book is blank.

**Distance of Nebulæ**

As regards the distance, a very important step has been taken by Sir William and Lady Huggins in the last ten years. In the Orion nebula, as in many other nebulæ, there appear to be involved various stars; but it was impossible to say whether these stars were in front of, in, or behind the nebula. By a skilful observation with the spectroscope, Sir William and Lady Huggins have made it most probable that the stars are really part of the nebula; for the spectrum of the star is found to closely resemble that of the nebula.

This being the case, it may be possible to learn something of the distance of the nebula; for though the delicate measurements neces-

sary to determine parallax cannot be made on the vague and indefinite image of a nebula, they can be made on a star; and if we can find the distance of the star we shall know that of the nebula. It is significant that this investigation has not (so far as I know) been yet undertaken; the attention of astronomers has recently been claimed in so many new directions that they cannot possibly do justice to all, and some of the most attractive problems have accordingly failed to attract solvers. The astronomical standing-army is a very small one, and much of it is wanted for home-defence—for keeping a watch on the objects already discovered, and doing routine work that must be done. It is nobly reinforced by volunteers; and there is a perfect accord between the regulars and the reserve forces. But we are in the presence of a vast extension of the astronomical empire, and we begin to find how small our numbers really are. Is it a vain hope that our ranks may be materially increased shortly?

# Section IV

# MODERN MATHEMATICAL ASTRONOMY

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## MODERN MATHEMATICAL ASTRONOMY

It was remarked at the beginning of the first section that new developments had recently taken place not only in practical astronomy but also in theoretical. To give any adequate idea of the mathematical progress of the last quarter of a century would be quite beyond the scope of a work like the present. But an attempt will be made to give a brief account of the *kind* of changes which have revolutionised theoretical astronomy; for they are quite as striking as any already mentioned.

**Planetary Theory**

There are two great historic problems in theoretical astronomy—the Planetary Theory and the Lunar Theory, as they are called. The first problem may be thus stated: if no other

bodies existed but the Sun and a single planet, it has long been known that the planet would describe an ellipse round the Sun under the influence of his attraction, the Sun being in one focus of the ellipse. The fact that the planets described ellipses in this way round the Sun *very approximately*, was discovered in the seventeenth century by Kepler from a study of Tycho Brahe's observations ; and the explanation of the fact was given by Newton in his famous law of gravitation.

But since other planets exist this is only an approximate and not an exact statement. The attractions of the other planets disturb this simple state of things, and the problem of the planetary theory is, how to find the actual motion of a planet when others exist to perturb it by their attractions. It is an extremely difficult problem, and cannot be completely solved with our present mathematical machinery. We are enabled, however, to get an approach to the solution for our solar system, because all the planets are small compared with the Sun, and their perturbations are minute compared with the main attraction of the Sun.

Hence, although any one planet does not describe persistently an ellipse round the Sun, as it would if all the others were removed, it follows this path very nearly for some time.

The small influence of the perturbations is manifested in two distinct ways:—

**Periodic Disturbances**

Firstly, instead of keeping accurately on the track prescribed by the Sun, the planet deviates now to one side and then to the other. Part of the attraction of the disturbing planets acts in contrary directions at different times, and so keeps the body oscillating, as a pendulum oscillates about a mean position. If this were the *only* effect of perturbations, the ellipse round the Sun, though not the actual orbit of a planet, would still be its average orbit, from which it would never be very far removed. The deviations from side to side are called "periodic inequalities."

**Secular Disturbances**

Secondly, the average path does *not* remain the same; it slowly changes, under the influence of a part of the disturbing attractions which always acts in the same direction, and thus steadily accumulates. The path deviates

more and more from the original ellipse, which ultimately ceases to represent the orbit even approximately. Such changes are called "secular."

**Illustration**

Perhaps an elementary illustration may help in elucidating these ideas. Suppose we have a steamer travelling on a perfectly flat, calm ocean, with the helm firmly fixed truly amidships. It will describe a perfectly straight line, and this we may call the undisturbed orbit, corresponding to the fixed ellipse round the Sun as focus described by a planet if no other planets exist.

Now suppose the helm is slightly interfered with. We may imagine two kinds of interference. In the first kind the helm is not *fixed* amidships, but is occasionally slightly to port, and as often slightly to starboard, in the sort of way that would occur in actual steering, owing to the irregularities of wind and wave. The steamer would take a slightly serpentine course, sometimes to one side of her "undisturbed orbit," sometimes to the other; but, on the whole, she would follow the same course as before, or at least would never deviate far from it.

If, however, the helm were by a slight mistake fixed *not truly amidships*, we should have a very different kind of interference. The steamer would no longer pursue a straight course, but would turn continuously, and so describe a circle. The smaller the deviation of the helm the larger would be the circle described; but in all cases (provided the ocean were large enough) the complete circle would ultimately be described, and the steamer would find itself sooner or later going over its old track. It would start very nearly along its old straight track, but there would come a time when the vessel was steaming at right angles to its original track; another time, when it was steaming parallel to it but in the precisely reverse direction, and so on. The whole character of the motion is gradually changed by this sort of interference with the helm, which corresponds to the perturbations of a planet called "secular."

Now we may proceed to use this illustration for the purpose of representing the way in which mathematicians were accustomed to treat the planetary and lunar theories, and the important change in method recently in-

troduced by Mr. G. W. Hill. The old method of considering the motion of the steamer may be stated as follows:—

If the helm were truly amidships the steamer would describe a straight line.

The error of the helm is very small.

Hence, the actual path will be *very nearly* a straight line, though this straight line will continually change in direction.

Let us, therefore, take the path as a straight line, but allow this line slowly to revolve; and let us apply ourselves to determine the rate of revolution, and then we can predict the place of the steamer at any time.

This corresponds to the old methods of planetary and lunar theory, which were by no means unsuccessful. By laborious calculations fairly satisfactory tables of the planets and the Moon were formed, and it was thought that the interest had almost evaporated from the problems.

**G. W. Hill's New Methods**

Suddenly, however, new life was put into them by a complete change of method, which allows of much less laborious, and much more

complete solutions. Mr. G. W. Hill's argument may be stated for the case of the steamer thus:—

It is true that if there were actually no error of the helm the steamer's path would be a straight line; but it is equally true that *the slightest* error changes this path into one of quite different character, viz., a circle.

It will, therefore, be better to accept the situation at once, and call the path a circle from the start. To cling to the notion of a straight line only hampers the work, and we can get on much better without this notion.

By actual experience this was found to be the case. The idea was a simple one, but fundamentally important; it has effected a revolution in the methods of the planetary and lunar theories; so that those who studied them thirty years ago in what was thought to be practically their final form, would barely recognise the modern treatment.

We will now repeat for a planet the statements corresponding to those given for the illustration.

The old methods started with the ellipse round the Sun, which is the undisturbed orbit.

The perturbations due to other planets being small, it was assumed that the actual orbit would be *very nearly* an ellipse, though the shape and position of the ellipse would continually change.

The orbit was, therefore, taken as an ellipse, *constantly, though slowly, changing in shape and position*, and the mathematicians set themselves to determine these changes, and thence to find the place of the planet at any time. The changes are, of course, far more complex than the simple turning of the steamer's path, but some of them resemble it closely. The axis of the ellipse, for instance, instead of remaining fixed in direction, as it would if no other planets existed, in actual fact revolves slowly in its own plane. So also the plane in which the orbit lies revolves instead of remaining fixed. The very smallest disturbing planet that could be imagined would make all the difference: *without* it the axis and the plane would remain permanently fixed, *with* it they would revolve, and, however slow

the revolution might be, it would ultimately carry them right away from their original position.

Now, although this idea of starting with the ellipse, the "undisturbed orbit," and following its modifications from perturbation, led to the solution of the problems of planetary and lunar theory with a considerable measure of success (so much so that thirty years ago it was felt that probably all the success had been attained which could be expected in such a difficult matter), still, G. W. Hill and other mathematicians have recently shown that it is much better to discard the ellipse at the outset. To cling to it is a loss rather than a gain, for a fixed ellipse is really different in essentials from a revolving one.

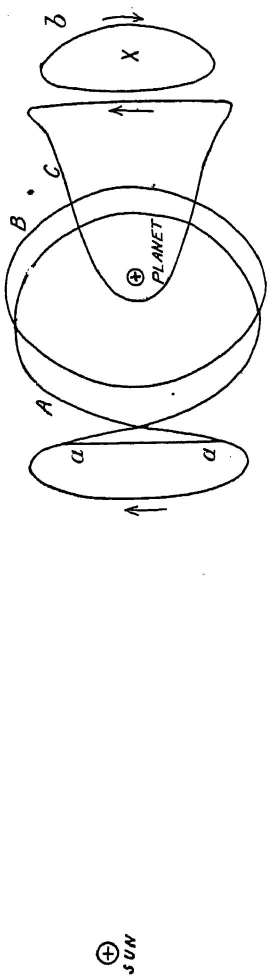
**The Variational Orbit**

It was in lunar theory, and not in planetary, that G. W. Hill first introduced this reform, but it applies equally in both cases. In lunar theory we consider the orbit of the Moon round the Earth. This would be an ellipse with the Earth in one focus if no other bodies existed; but other bodies, and especially the Sun, perturb this orbit. The perturbations are small, even for the Sun; for though he is very much

larger than the Earth, he is so far away as to more than counterbalance his size. Hence, the problem is in many respects similar to the planetary problem: to find the small disturbances produced by a third body. It is found possible to separate the disturbances into several types, and one important type is called the "variation." Hill included this in his first approximation, and hence the name "variational orbit."

It would be difficult to attach too much importance to this simple but fundamental reform: others who have worked in the same field have testified emphatically to the fruitfulness of the idea. M. Poincaré, who received the gold medal of the Royal Astronomical Society in 1900 for his splendid work in mathematical astronomy, wrote of Hill's papers, "Dans cette œuvre il est permis d'apercevoir le germe de la plupart des progrès que la science a faits depuis." And these words were quoted[1] with approval by the President of the Society, who had himself received the medal for work in neighbouring fields, in his address on the occasion.

[1] *Mon. Not. R.A.S.* vol. lx. p. 413.




SOME POSSIBLE "PERIODIC ORBITS" (G. H. DARWIN).

**Periodic Orbits**

One of the most striking advances which have resulted is represented by the study of "periodic orbits." The actual orbits of the Moon and planets are all very nearly circular; and the methods of the old lunar and planetary theories, designed as they were with reference to these actual cases, were not capable of extension to cases differing very much from them. But it is easy to imagine cases essentially different, though mathematicians had until

lately been able to do little or nothing in the way of studying them. Hill first showed how to get some general information on the subject, and from his work, and that of G. H. Darwin, Poincaré, and others, we are gradually getting a general analysis of the possible orbits which may be described by one body in the presence of *two* others: orbits entirely unlike ellipses or circles, as will be seen by reference to the diagram taken from one of Professor Darwin's papers. It lay quite outside the old planetary theory to consider even for a moment a "figure of eight" orbit, such as that marked A, or a bell-shaped orbit, like that marked C, or an orbit such as that marked *b*, where the satellite or Moon does not go round the Earth or Sun at all, but oscillates outside them! The mathematician will, on seeing such orbits drawn, naturally ask about the stability; and, as a matter of fact, none of those drawn in this diagram are stable; but they are possibilities, and we cannot get a complete idea of planetary motions without paying some attention to them. They may even be actually occurring in Nature more often than we think. It has been suggested by Gyldén, and recently quite independently

by F. R. Moulton, that the Earth has a number of satellites of the oscillating class *b*—very small satellites, such as when they fall on the Earth we call meteors, but making up in number to some extent for what they lack in size. The suggestion was made to explain the phenomenon known as the "Gegenschein" or counterglow. There may be seen on a fine, dark night an extremely faint patch of light in that part of the heavens just opposite the Sun—that is, in the direction from the Earth to the cross within the orbit *b* in the diagram. What is the explanation of this appearance? Gyldén suggested that it is the sunlight reflected back to us from a number of meteors at that spot, which are not absolutely fixed, but are describing in the neighbourhood of the cross oscillating orbits like that marked *b*; and since these orbits are unstable, any particular meteor does not remain permanently in the neighbourhood, being after a time swept away in some different orbit; but this is compensated probably by the arrival of another meteor, which joins the general dance for a few turns. Near the cross there is always therefore a little crowd of meteors, each of which is arrested for a time in the midst of its more

**The Gegenschein**

extended travels, to jig up and down, backwards and forwards, for such time as will keep the "pot a-boiling," or, rather, the Gegenschein a-shining. The idea is, so far as can be seen at present, a satisfactory explanation of the phenomenon, and is the first application of the new investigations on orbits hitherto unstudied. There may be many others to come; for although the orbits of our own solar system are all nearly circles, there are among the stars cases of three or more bodies forming a stellar system; and when we know more of their motions we may find all sorts of curiosities in the way of orbital motions. At present such systems have not been watched long enough to tell us much of their complete orbits; and it may be added that we have scarcely had time to apply the new ideas to observations already made. Here, as elsewhere in astronomy, we are only on the threshold of the new departure.

**Forms of Planets**

The lunar and planetary theories, although their methods have lately been revolutionized, were already worked out with more or less success. But there is another department of mathematical astronomy in which very little

had been done before the last quarter of a century, and in which a good deal has been done since, viz., that which treats of the changes of form of the heavenly bodies in long periods. A few elementary propositions on the "Figure of the Earth" had been arrived at, and the mathematics of the tides had been studied and found extremely difficult; but any one who takes up Professor G. H. Darwin's book on "The Tides"[1] will see from the references there given how much of the work in this new department of astronomy is quite recent. This book is an admirable popular exposition of such work, and its existence renders any extended account of it here more than unnecessary. I will, however, venture to give, in illustration of the kind of work referred to (much as a reviewer might quote from the book he is reviewing while recommending it for full perusal), the striking results arrived at by Professor Darwin regarding the past history of the Earth and the Moon.

**History of Earth and Moon**

According to Laplace's nebular hypothesis, the whole solar system has been generated

[1] *The Tides and Kindred Phenomena in the Solar System.* By George Howard Darwin. London: John Murray, 1898.

from a single nebula, the greater part of which now forms the Sun. As this nebula contracted from its original diffused form, rotating faster and faster (according to the well-known law of conservation of moment of momentum), it threw off rings which broke up and formed the planets. In the same way these generated their satellites, circulating round them as the planets circulate round the Sun, and so the Moon was formed out of the Earth.

**Effect of Tides**

Now, we know that the Moon causes tides on the Earth, and though we usually think of tides as affecting the ocean only, it has been recognised recently that there must be tides in the solid Earth as well. The same forces of attraction which are able actually to move the liquid water, must produce strains in the solid earth, and though the effect may be small now, it would be larger when the Earth was more plastic, as it must have been if it was formed by condensation from nebulous matter. Further, not only does the Moon cause tides in the Earth, but the Earth could cause tides in the Moon. It does not cause ocean tides, because, so far as we know., there is no water on the Moon; nor does it *now* cause bodily tides in the

solid Moon; but this is merely because in time past it caused such enormous tides of this kind that it has reduced the Moon to a state where they are impossible. The tides, in fact, whether ocean or bodily, caused by a satellite in its planet, or a planet in its satellite, gradually cause changes in the relative motions of the pair, and in their distance apart. There are "strained relations" between them, in consequence of which they gradually separate, and the rotation of each is modified —especially that of the smaller. The effect on the rotation is this: each of the pair tries to make the other rotate on its axis in the same time which is occupied by the revolution of the pair round each other. In the case of the Earth and Moon we call this time a month, and the Earth, being the bigger and stronger, has already succeeded in making the Moon revolve on her axis in exactly one month; so that we always see the same face of the Moon. The Moon is trying to do the same thing to us, and will ultimately succeed no doubt. At present the Earth rotates in a day—very much faster than the Moon likes: she is reducing this speed as much as she can, but her power is very small, so small that we can scarcely

**Alteration of Day and Month**

detect any appreciable diminution of speed in historic times. But the diminution must be there, and will ultimately make our day a month long. The Earth will then always turn the same face to the Moon as the Moon does to us; and the Americans may have a monopoly of the Moon—they may see it always, while it will never be seen in Europe, or *vice versâ*. How far ahead this is we cannot say; for we can scarcely measure the rate of diminution of the Earth's velocity as yet: we must wait for a little more information.

But it is easier to go back in time than to go forward, because the effect was greater in the past than it will be in the future. And following up such clues as are available, Prof. G. H. Darwin has made a very fair guess at the past history of the Earth and Moon. Some years ago he gave in one of his papers the following interesting little table of dates. It must not be taken too accurately, but it gives at least a general idea of past events :—

| Sidereal day in M.S. hours. | Moon's Sidereal period in M.S. days. | No. of days in Month. | Moon's distance in Earth's mean radii. |
|---|---|---|---|
| h | d | d | |
| 23·93 | 27·32 | 27·40 | 60·4 |
| 15·50 | 18·62 | 28·83 | 46·8 |
| 9·92 | 8·17 | 19·77 | 27·0 |
| 7·83 | 3·59 | 11·01 | 15·6 |
| 6·75 | 1·58 | 5·62 | 9·0 |
| 5·60 | 0·23 | 1·00 | 1·5 |

s of the day and month have been because of this influence of each e time of rotation of the other. It n from the second column of the the Moon's recent influence on the ne of rotation is, however, slight: forty-six million years it has only e day from a fifteen-and-a-half hour venty-four hour day. Going back s time the change was more rapid. nost interesting columns are the fifth. Looking at the bottom of column, which refers to a time like fifty-seven million years ago, ne day in the month only, *i.e.*, the unning round the Earth as quickly er rotated on its axis. This must the time when the Moon was just

**Separation of Moon from Earth**

flung off the Earth, and is an interesting birthday. We see from the fifth column that her distance from the Earth was then only one-and-a-half radii, so that the Earth was only very little greater than its present size. It has since contracted in size, and the mutual tidal action of the two bodies has carried off the Moon to its present distance; but it is interesting to see how these figures come out. The table is the result of most laborious calculations, and several hypotheses have to be made in the course of them. If Professor Darwin had got a result at the end which was impossible or absurd, it would have been disappointing, but perhaps only such a disappointment as he should have been prepared for. If, for instance, the last figure in the fifth column instead of coming out 1·5 had come out 0·9, meaning that the Moon separated from the Earth when the latter was *less* than its present size, the whole work would have been discredited, for we know of no causes which would produce gradual *expansion* of the Earth as time went on. But the figure 1·5 is eminently reasonable, and we may accept with some confidence the result that the Moon separated from the Earth when the latter was

not much bigger than now, and about fifty-seven millions of years ago. The date is subject to alteration when we have measured the present rate of tidal retardation more accurately, though it is unlikely to be reduced below twenty millions, or extended to more than one hundred millions. But those interested in such glimpses into the past should certainly refer to the book itself, where they will find it stated clearly, and with due caution, what is the best information we have yet acquired.

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